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EDITED BY J. MCKEEN CATTELL

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PSYCHOLOGICAL HYPOTHESES CONCERNING THE FUNCTIONS OF THE BRAIN

By Professor KNIGHT DUNLAP
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SINCE the fifteenth century, the philosophers who have discussed the human mind have based their theories on two concepts, namely, the soul and brain. With the rise of physiology in the nineteenth century, the brain assumed still greater importance, and the phrenological conceptions of Gall and Spurzheim led to conceptions of localization of mental functions in the brain which seemed to give that organ a detailed importance in place of the general importance it had held. One has merely to go back to the writings of the philosophers and physiologists and psychiatrists from 1850 to 1900 to recognize the positiveness with which psychical processes were assigned to the operation of brain cells. The proponents of interaction couched their arguments quite frankly in terms of certain hypothetical items of "consciousness" which they called sensations, images and feelings, and which were claimed to be products of cerebral action. The parallelists, on the other hand, objected not so much to this notion as to the converse notion that consciousness in some way acted causally upon the brain cells. It is metaphorically easy to see how a brain cell by its action might secrete consciousness as a rose sheds attar or a harp string music. That a "sensation" or an "idea" could have energy to excite or modify the "material" action of a cortical cell seemed to these allegorists to be a less plausible supposition. Yet the parallelists accepted the cerebral processes as the only bodily processes with which this consciousness was directly connected, even if the connection was denied to be a causal one.

Even as late as 1925 an American neurologist who occasionally breaks into popular philosophizing expressed himself as follows:

> We do not know exactly how a sense organ is excited, how a nerve fiber conducts, how a muscle contracts, how a gland secretes, or how the brain thinks, although we have satisfactory evidence that all of these organs do perform the functions mentioned. No biologist with all the evidence before him can fail to make this deduction. Why some other people accept the evidence in all of the cases except the last and refuse to do so in that is hard to understand. Presumably it is due to lack of knowledge of the biological evidence or else to a mind fortified against this evidence by prejudice.

When experimental psychology began to develop, in the latter half of the nineteenth century, the soul was slated for rough treatment, and all but a fraction of one per cent. of psychologists since that period have refused to involve the soul in their problems or their methods or their conclusions. This revolt against the soul was in no sense a philosophical or a religious movement, but a purely scientific insurgence. The term soul has literally no precise meaning, because it has a plurality of different meanings

which are utterly confused by the persons who use the term; and science can not proceed by the use of meaningless terms and jumbled concepts.

If you consider only a few of the concepts of the past and present, you find them a motley aggregation. You find the psyche, or unindividualized life force; the ghost or shade; the indweller; the double or "other one" (the Ka of the Egyptians, which seems really to have been the placenta); the ego; the "personality," and such ilk. You will find these concepts startlingly different from one another, and you will not be willing to affirm or deny the existence of the soul until it has been made precisely clear what it is that you are affirming or denying. You will also find that the perennially engrossing question of immortality is a vastly different question according as it is tied up with this, that or the other kind of soul, and that millions of people have believed in immortality while denying the existence of any sort of soul.

With the putting of the soul out of psychology, a vacancy was created into which a series of substitutes have crept. The case is somewhat like the one presented in the parable of the devil which was cast out of a man, but which shortly returned, bringing a number of other devils with him. "Consciousness," which as its form indicates is an abstract term, became for the soulless psychology a concrete noun, designating a kind of thing, or observable something, which was made the basis of the mind, as the soul had formerly been. There is a pernicious tendency on the part of man to seek to explain phenomena by pointing to an assumed substantial something behind or under it, as the Hindus sought to explain the foundation of the earth by assuming an accommodating elephant on whose back it rested. Even to-day, when the term consciousness has, through the efforts of modern psychologists, been restored to its abstract meaning, there are small groups of fundamentalists who still speak of consciousness as though it were objective and observable, and a greater company of biologists and physiologists who believe it to be a kind of quasi-objective reality.

The soul is a hard customer: in spite of our excommunication it is difficult to keep it permanently outside the fold. Like the repressed desire of the Freudians, it is constantly sneaking back, disguised under some other name. One might suppose that "intelligence," after the thorough confusion wrought upon it by philosophers and biologists, and "intelligence tests," would be harmless terms, obviously abstract and tentative. The discussions of the question whether intelligence is one or many, if you attend to the actual significance of these discussions, should disillusion you. Intelligence, in some of its conspicuous usages, is just our old friend the soul again, merely divested of its thaumaturgic and vitalistic draperies and wearing statistical false whiskers.

We have with us also the good old soul masquerading in a cloak of many colors under the alias of personality. Some of those who revere intelligence revere the personality also; but in general, those who worship at the shrine of the latter ignore or slight the altar of the former. In spite of the fact that the more experimental of the psychologists use the term personality strictly to describe the impression which the individual makes on others, there is unmistakably a group who, dissatisfied with our dealing with the individual's perception, thought, feeling, actions and habits, insist on dealing with these all over again from the point of view of "personality traits"; and who obviously deem that in so doing they are getting closer to the vital core of the individual's mental life—to his soul, in other words.

Even with consciousness, an intelli-

gence or a personality as a substitute, the passing of the more robust concepts of the soul from psychology threw a greater emphasis on the brain, and experimental psychologists, following the lead of the physiologists, sought to find the secret of the mind in the chemical operation of the cerebrum, and stated their problem and results indifferently in factual terms of actual occurrences of perception and thinking and feeling, or in terms of hypothetical brain states and cerebral processes, or more frequently, indeed, mingled the two terminologies in a kind of scientific hash.

We accepted the conventional sensory centers in their phrenological signification, and solemnly sought for thought centers and feeling centers and for other centers corresponding to more recondite classifications of mental facts and occurrence. Within the year, a European physiologist has achieved notoriety, at least in America, by announcing the discovery of the sleep center. The obvious mental defects of those who have suffered from pathological brain development and from brain disease and trauma have fitted into the brain-center scheme in a general way, and have been made to fit more specifically in fearful and wonderful ways, much as Neolithic man fitted his corpses to cramped burial bowls. With the emphasis on mental heredity, it was early assumed that this heredity was determined by the type of brain inherited. If musical talent "runs in a family," then a "musical brain" must be hereditary in the family stock.

The experimental method, however, became too vital a part of psychology to permit the long continuance of this situation. All this schematization eventually appeared to be what it really was, namely, an elaborate interpretation of preciously slight experimental fact. The work of Sherrington had opened new vistas of possibilities. The psychologists were restless and ready for migration to pastures more promising than the phrenological, as soon as a practicable route should be opened. In America, this pioneer work was done by Shepherd Ivory Franz, whose experiments on the recovery of vision in monkeys in which the visual centers had been permanently put out of operation, and in the reeducation of tabetics, showed the way.

Since that time, of course, there have been those who have gone to the opposite extreme in interpretation of experimental results. Little has been added with certainty to the indications Franz was able to outline, although there has been some hazardous drawing of specific conclusions not justified by the data.

Justifiably or unjustifiably, the psychologists' conception of the function of the brain has changed, and this change is the most significant single reformation in the history of the science. The new formulation of the psychologists seems also to have affected the workers in the biological sciences. Even the eminent neurologist whom I quoted a moment ago now puts his theories in a quite different style, much more in accord with psychology.

In brief, the new working hypothesis in regard to the brain can be reduced to a series of statements.

(1) The brain is an integrating organ, and we know of no other functions which it has.

(2) The brain "centers" are anatomical, and not psychological.

(3) There are no known differences in the kind of function between cells in different parts of the brain. (There may be differences in the time-relations, energy and stimulation of these actions.)

(4) There are no known types of function possessed by cells in the cortex which are not possessed by peripheral neurons, although there are types of functions possessed by certain peripheral neurons (the selective stimulability of receptors) that are not possessed by central neurons.

(5) Consciousness is an abstract characterization, not of brain cells, or even of the brain as a whole, but of response, that is to say, reaction. Put concretely, certain responses are conscious. The brain actions, taken by themselves, are neither conscious nor unconscious.

(6) "Conscious" is merely a descriptive adjective applied by convention to such responses as hearing, seeing, thinking, etc. These processes are so named by common consent, and the naming is in no wise an explanation. No one, not even among the former behaviorists, denies the occurrence of seeing, and of a variety of processes which seem properly classified therewith; and the calling of these conscious is a mere matter of useful convention, like calling certain mechanical actions explosive. Any other term would do as well, if generally employed.

The best simile I know to express the relation of the brain to response, consciousness and unconsciousness is the simile of the telephone exchange system. The brain is a system of central stations, connected with one another by trunk lines and connected each with a multitude of subscriber's stations by afferent and efferent neurons. So long as the lines are intact and the central stations in normal condition, any subscriber can call any other subscriber in the city. Here the simile breaks down, because in the telephone system, the subscribers who are called can call back over the same lines (*central volens*), whereas the connections from receptor to effectors in the animal body are one-way lines. The receptor can call, but can not be called except over a different line. The telephone system is, in a way, more efficient.

In another way, and a very important way, the nervous system "has it over" the telephone system, since from a single receptor a multitude of effectors can be innervated at once, and conversely, a multitude of receptors can cooperate in the calling of the same effector.

This brings us to the really important integrative feature of neural action. Within certain limits, the nervous system acts as a whole in conscious response. Here our analogy breaks down completely. In describing a sample reaction we speak conveniently of the stimulus as if it were the action on a few receptors, and the response as if it involved only a relatively few afferent, cerebral and efferent neurons and a few muscles. But we know that the real stimulus is a pattern involving practically all the receptors in the body; and the real response involves the nervous system integratively, and practically all the muscles, and many, at least, of the glands. Even such a response as the knee-jerk in the uninjured animal can readily be shown to be no mere reflex but to depend on a vastly greater assemblage of receptors than those terminating in the patellar tendon, and I am confident that within two years we shall be able to show that all the muscles in the skeletal system participate in this so-called reflex, and in all other overt responses of the human body.

In this integrative system, the visual cortex is "visual" only because it happens to be directly connected with the retina. Normally, no visual stimulus can affect the organism, in more than a limited local way, unless the connection between the retina of the eye and the skeletal muscular system be intact; and perhaps the smooth muscular and the glandular systems are important. Experimentally, the connection can be broken in any one of four ways: (1) By destroying the retina; (2) by cutting the optic nerve; (3) by cutting the optic tract; (4) by isolating the occipital cortex from the remainder of the brain.[1] In each case the result is the same. The animal is blind. Theoretically, the same result would be obtained by cutting all the efferent fibers running from the brain to the muscles and glands, but this experiment can not be

[1] This was Franz's method.

tried, and I strongly suspect that it would not succeed unless the cerebellar connections were cut also. Seeing requires a functioning and variable connection between the retina and the muscular system. If, however, after destruction of the occipital cortex, the retina can could be functionally connected to some other part of the cortex, vision will again occur, although perhaps not as efficiently as before. An analogous condition is again found in our telephone system, where, if your exchange is destroyed by fire, the cables are cut over to another exchange, and your phone is again alive, although the crowding of the new exchange may interfere somewhat with efficiency of operation.

Although recently published conclusions maintain that in the rat, the destruction of the occipital cortex does not materially decrease the efficiency of visual discrimination of a crude sort after sufficient time has been allowed for recovery from the operation, the data which are offered do not seem to support the conclusions. Nor, in fact, are the data so far sufficiently critical to support any definite conclusion. But even if certain responses analogous to tropisms or simple reflexes do occur after the recovery from operation, we must not expect to find the more delicate performances resulting from the same sensory type of initiation as efficient as they are when the brain connections are normal. On this point, as on the other points of interest in animals who have recovered from brain operations, much carefully and accurately controlled experimental work needs to be done before conclusions can be drawn. In addition, histological methods must be applied, so that we may know what has really happened in the brain of the animal in the process of recovery from the operation. Professor Pike has trenchantly stated these requirements in his recent note in *Science*.

The function of nerve cells, inside or outside of the brain (always excepting the receptors), can apparently be reduced to a single category, so far as present knowledge is concerned, namely, to be irritated, or excited, and to discharge and so irritate other cells. Differences in thresholds of irritability, differences in energy of discharge, differences in the temporal features of discharge—these there may be, and there are differences of spatial interconnection to be reckoned with; but other differences we wot not of.

The negative factors in this scheme are drawn from neural anatomy. The positive factors are the result of our increasing knowledge of discriminatory perception, by which it has been made apparent that a difference in perception, a perceiving of this as distinguished from that, depends primarily upon the building of a difference in muscular response.

One can not discriminate between two objects, or between two situations, unless one can make a reaction to the one which is different from the reaction to the other, and can make these reactions in such a systematic way that the presence of the one object or situation evokes the one response, and the presence of the other object or situation evokes the other response. These responses may be of a general or of a specific type. They may be, for example, movements of avoidance and approach, or they may be vocal movements, producing spoken words. The specific response is the more scientifically useful, and the vast vocabulary of words at our disposal together with the constant necessity of inventing new technical terms as new discriminations are required are living testimonials to the basal characteristic of discrimination.

From this situation there results a consideration which at first appears to complicate the problem, although it may

explain certain operative results which seem otherwise to offer difficulty. We know that perception is a matter of habit formation. The regularity and consistency of stimulus patterns builds up, through successive responses, types of response which are perceptions of specific objects. Stratton's experiments with inverted vision show that it makes no difference whether the image on the retina is erect or inverted, provided it is consistently one way or the other for a sufficiently long (really a surprisingly brief) period. Other laboratory experiments with prisms described by Sanford give results of the same order. It is to be assumed, therefore, that if regular stimulation of a certain brain area by nerve current from the retina, and the retina only, builds up the responses which become perceptions of light and color, a sudden change in stimulation by which these cortical cells are stimulated from sources other than the retina may produce a somewhat similar afferent and muscle pattern, and so evoke perception of the visual mode, without visual stimulation. This would perhaps be a temporary effect, and may explain the results which have been reported from the experiment of stimulating the cortex directly by electric currents. However, I should wish these experiments repeated under careful psychological control before leaning heavily upon them.

The explanation of imagination of visual, auditory or other modes, where there is no effective receptorial stimulation, does not offer difficulty. Such evidence as we have tends to show that in having a visual image the observer makes a response which terminates in a muscle pattern which has habitually followed a stimulus pattern of the type he is imagining. For example, in imagining any specially definite object, Totten has shown that the observer makes eye movements of an appropriate type. Perhaps many other muscles are employed, and experiments now in contemplation will eventually supply evidence on this point. It is not necessary to assume that the visual cortex is excited in visual imagining, or that the auditory cortex is excited in the production of auditory imagery. Nor need the destruction of the normal visual or auditory "centers" permanently deprive the animal of visual or auditory imagery, even if the eye does not resume its function.

We are approaching at this point a most important problem, namely, the extent to which actual muscular activity is necessarily involved in conscious responses. I have certain notions about this, but some acute experimenters are at work on the muscular problem, and it will be advisable to await their results. There are a host of fascinating problems connected with this one, but if I am to get to my main point within reasonable limits of the reader's time I must resist their fascinations and pass on.

The nineteenth century emphasis on the brain as the source of mental life naturally tended to identify individual differences in mentality with differences in the constitution of the brain. The man of superior mental ability may well be supposed to have a better brain, and the low-grade moron may reasonably be assumed to have a low-grade brain. The expert mathematician has a mathematical brain, and the clever writer a literary brain. Most of us have just common, mediocre brains.

A short time ago, an ambitious young psychologist seemed to have demonstrated that with superiority and inferiority of mentality went superiority and inferiority of the spinal reflexes; superiority and inferiority, at least, in respect to speed of action. Measurements of the time of the knee-jerk in groups of people ranging from moron to highly intelligent, when correlated with intelligence-test scores of the same

persons, gave coefficients of correlation of significant size. "Mentally quick, speedy kick," was the indication. Most psychologists were skeptical, as the details of the experimental report were not clearly satisfactory, but the conclusions were nevertheless heralded as one of the greatest discoveries of the age. Unfortunately, the skeptics' expectations have been confirmed, and it is informally reported that more careful experiments show only a chance correlation, and the brain's aristocratic position is so far unshaken, although there is no reason to suppose that high speed of reaction is a uniform characteristic of either spinal or cerebral efficiency.

The relation of brain structure and organization to mental ability receives confirmation from pathological and surgical observations. Loss of a considerable portion of the cortical substance, except from the frontal region, profoundly affects the mental characteristics of the patient, even though no specific sensory alteration be produced. Degeneration of tissue likewise produces mental degeneration. Microcephalic and hydrocephalic individuals have impaired mentality.

For the greater range of individuals, however, from medium-grade morons to geniuses, no characteristic differences in cortical tissues are discernible. Here, the difference may still exist, and there may be characteristic differences in the brains of mathematicians and prizefighters, bankers, politicians and priests, in spite of the fact that research has not yet shown them.

For the low-grade moron, however, examination does seem to show a structural peculiarity, consisting in a deficiency of interconnection of the cortical cells. This is a fact of profound importance which fits squarely into our integration theory, and also into the heresy I shall propound in a few minutes.

The general hypothesis that mental differences of capacity involve corresponding differences in brain constitution or organization may then rest unchallenged for the present. All lines of evidence converge towards the confirmation of the hypothesis. We can not escape the conclusion that where two responses differ, there is difference not merely in receptor action, and effector action, but in brain action also. Where systems of response differ in characteristic ways, there must be characteristic differences in brain action, no matter what these differences are due to. This, by the way, is one of the reasons why we may confidently expect that any readjustment of brain following operative injury will show its effects in some way in the response.

Brains differ, and the important question is: How do the brains get that way? For the discussion of this problem we may reasonably ignore the obviously pathological brains; those which have lost substance, have suffered syphilitic or other degeneration, those whose arteries are hardened and those demonstrably badly constructed from birth.

It is fairly certain that the important differences in brains of the normal range, including the brains of most geniuses, morons, criminals, saints and *hoi polloi* generally, are differences in organization, histological or chemical in character, and not anatomical in the gross sense. It is true that since Gall and Spurzheim many earnest investigators have thought to have discovered anatomical cerebral characteristics corresponding to mental traits, but these discoveries do not seem to stand the test of time and scientific investigation, but like the bumps of the phrenologist and the facial and other character-signs of Lombroso, Blackford and Company vanish into thin air.

There have been those who have thought the total size or weight of the brain was indicative of mental caliber. This thought was especially comforting

to the males, when it was pointed out that the average weight of the male brain is greater than the average weight of the female brain. Perhaps the emphasis on the fact that, relative to the body weight, the female brain is the heavier has largely contributed to the elimination of the brain weight hypothesis, although there are still anatomists of high scientific standing who think there is something in it. Possibly there may be; but there are so many other unanalyzed factors involved in the cases put forward that the psychologists are justified in their skepticism.

Others have studied the convolutions of brains, and have thought to find in their conformation or relative development the indications of mentality. These theories, again, may have something in them—it is almost impossible to prove a negative—but the probability is too small to cause us serious concern. That the cephalic indexes, once deemed so important by the anthropologists, have a significance in this connection is no longer seriously maintained.

How, then, does the individual human brain get its differential characteristics? A number of alternative answers might be suggested. First, we might say, the differences are inherited. The mathematician's brain has a peculiar organization which is inherited from ancestors whose brains had the same characteristic, and the moronic brain is likewise a heritage from moronic ancestors. A decade or two ago, in the vogue of a sort of heredity of almost fatal character, this view was held with an extremeness which seems incredible to us now. I believe that the biologists were primarily responsible; but anyhow, the psychologist went so far as to assume that education, and the environmental influences generally, had almost nothing to do with the organization of the brain. The moron might, through special training, learn to use his poor brain a little more effectively than he otherwise could, but its moronic character was supposed to be established by a simple process of growth from the originally incompetent germ-cells. And so on, for the total range of differential characteristics. Heredity, however, to-day, is not what it used to be, and perhaps it never was. We no longer set heredity over against environment, nature against nurture, instinct against habit; but we conceive each in terms of the other. In this reformation, the psychologists have been with the leaders of the procession.

The second extreme answer to our question is the hypothesis that (still considering only the non-pathological brain) the differential characteristics are due entirely to education or training. This answer is not in good form to-day. The majority of psychologists, and of biologists also, I think, incline to take a compromise position, holding that training is influential in determining the organization of the brain, but that heredity establishes the limits.

This is nearer the truth, but still implies a false conception of both heredity and environmental influence. A sounder form of statement, for which I received sharp criticism a few years ago but which I think agrees with Jennings' formulation and which is rapidly gaining ground, is to say that nature and nurture are mutually inclusive. Hereditary tendencies are not absolute, but are relative to the environment, and are capable of formulation only in terms of the environment. Environmental influences, on the other hand, operate only on hereditary tendencies. Each is a function of the other, the two being comparable to mathematical factors in a product, which accordingly varies with both and becomes zero when either factor becomes zero.

This formulation, however, leads to the consideration of a possible case which is most interesting. If either factor in a product has a constant value of unity, then all variations in the

product are determined by the variations in the other factor. At this point, therefore, I wish to introduce a hypothesis which, from a point of view of the unanimous conservatism heretofore obtaining on the point, is nothing less than rank heresy.

As regards mental characteristics and capabilities, I should maintain the conventional point of view, as expressed in the most modern formulations which I have just outlined. These characteristics vary on account of differences in heredity and differences in environment. But as regards the brain, I shall claim that the actual differences in normal individuals are due to differences in training alone; in other words, the direct hereditary factor has, for practical purposes, the constant value of unity.

This hypothesis is the logical consequence of that which is more and more becoming the working hypothesis of psychology, namely, that the brain is not the sole or even the primary "organ" of the mind, but is a contingent and subservient part of the mechanism.

I am insisting, in short, that so far as the brain itself is concerned, one brain is equal to any other brain for purposes of training; and that, although different brains are, at a given time, different, so far as practical organization is concerned the difference is due to differences in the actions of the brains' environments.

Let me distinctly state, however, that this commits me to no behavioristic position. I do not say that it is possible to train any individual to the point of mental and bodily capacity to which it is possible to train some other individual; I do not say that one individual is equal to any other individual for purposes of training. I am speaking so far solely of brains. To say that you can take any child, however young, and make a mathematician or a musician or a poet or a mechanic of him by any practical method is against the present evidence and is a proposition not to be seriously considered.

To say that the feeble-minded individual might have been trained to be a normally intelligent person is equally absurd, unless you extend the term training to include such operations as the fundamental modification of metabolism and the administration of glandular extracts. Ordinarily, we do not include these under training. But if you do so extend the term, then the statement is not quite so absurd.

The training of the individual is primarily the training of his brain. Other tissue can be trained to a certain extent, but the brain is the trainable organ *par excellence*. This holds for brains in school, in the shop, in the gymnasium and in business.

Training processes originate in the environment. But between the environment and the brain there are mechanisms which must not be overlooked. These mechanisms include the receptors, the glands of inner secretion and the muscles. The muscles are responsive to the environment through the brain, but their activity in turn stimulates the brain anew. We may not subscribe to any of the various specific theories as to endocrine action, but we must admit that the responses to receptorial action are modified by certain of the hormones, and the secretion of these hormones is in turn controlled by responses. Hence training, whether we speak of the individual as a whole, or of the brain alone, is carried on primarily through response. The response theory on which psychology to-day is solidly grounded irresistibly pushes us to the conclusion that the response tendencies are modified mainly by responses, and training of any sort is a modification of response tendencies.

The brain can be trained by the environment only in so far as the peripheral mechanism is capable of mediating the

training. If the practical limitations of training are not set by the brain itself, they are necessarily set by these peripheral mechanisms.

The premises so far are merely corollaries of the present working hypothesis of psychology. There is, however, an implicit assumption which must be made explicit, and this assumption is perhaps the only new feature of my radical position. It is the assumption that every normal brain possesses, in infancy, potentialities in excess of the development in actual life, even in excess of the development of the brain of the best individual.

There are doubtless individual differences in these potentialities, but they are so far outside of the range of development that they do not practically matter. To illustrate the point. The brain of every non-pathological individual is early capable of being developed, in mathematical capacity, beyond the capacity of the brain of the most brilliant mathematician; but few have the peripheral mechanism capable of administering the training. The brain of the low-grade moron was, in its early stages of development, perfectly capable of developing into a brain equal to that of the highest genius; but the peripheral mechanism was defective. If I may again resort to a crude analogy, the situation is like that of a multitude of motor cars with gas tanks of capacity ranging from nine to thirty gallons. But if the purses of the drivers are limited to the purchase of from two quarts to three gallons of gas, the differing potential capacities are practically of no importance.

I do not claim that there are different initial capacities of development of normal brains. I claim merely that if there are such differences, they are of no practical consequence for actual possibilities of development.

This assumption is not a wild one. There are many curious facts which suggest it, of which I shall mention only one here: the fact that in every brain there is an excess of neurons over those actually brought into functional operation. While number of brain cells is not perhaps the most important determinant of brain potentiality, it is undoubtedly of some importance.

If I am not mistaken in my diagnoses, psychology is very definitely on the road towards the consideration of the brain as an essentially plastic organ, especially in infancy and early youth, capable of meeting any demands which the receptors, the muscles and the glands are able to make upon it systematically. The development of muscular systems in the past seems to have depended upon the requirements of adaptation to environment, and the development of receptorial types seems to have depended upon a very definite requirement, namely, the existence of two sorts of stimulus, definitely related and grounded in the same spacial points or units. The failure to develop receptors for radio waves, for example, seems to have been due to no limitations of human brains, but to have depended upon the fact that these radiations, although they must have been present to the organism from the beginning, have not been systematically related, in the physical scheme, to any other forces playing upon the organism. There have therefore been no ways in which these radiations could be interrupted and made useful to the animal.

Functionally, the brain seems to be an integrative or interconnecting organ, and nothing more, except that its integration is habitual. More specifically, the brain by its multiplicity of synaptic connection enables any receptor to stimulate any muscular and glandular element. Its integrative action has then four features: (1) It enables the same stimulus-pattern at different times to produce different results; (2) it enables

different stimulus-patterns at different times to produce the same result; (3) it tends to fix the same result to the same pattern, and (4) it tends to develop fixed difference of result from differences of pattern which are practically important. Perception and emotion or feeling fit into the scheme perfectly. Thinking seems to offer some difficulty. In perception, the stimulus pattern produces a response which has become mechanized. In thinking, the presence of sensory stimulations as essential initiatory factors is not yet demonstrated. We are here offered two alternatives: (1) the implicit muscle pattern theory, which assumes that in every thought process there is an actual muscle pattern, the end result of a preceding response; and that this muscle pattern, although it may not be demonstrable by ordinary means, is nevertheless effective as a stimulus. (2) The muscle substitute theory, which, while admitting the importance of the muscle pattern in general, claims that the muscle pattern in many cases drops out entirely, and the afferent impulse is supplied by a correlated mechanism which has been trained to produce the same brain pattern (and therefore the same terminal muscle pattern) which has been produced by the former muscle pattern. This substitute mechanism can be no other than the cerebellum, and hence I conceive of a train of thought, of a habitual sort, as essentially a series of discharges between the cerebellum and the cerebrum. Decision on this point rests upon the completion of much work, including the galvanometric work on the detection of faint action currents of the muscles, upon which Dr. Max has been engaged for several years.

It is to be emphasized that if this view is correct the brain supplies no initiative in our responses, conscious or otherwise. It supplies merely connections and conservation, *i. e.,* habit. The brain has no product called consciousness; at least, we know of no such product. Consciousness is an abstraction; consequently, we can't reasonably ask what it is or where it is located. The term conscious is purely a descriptive term applied to the processes of perception, feeling and thinking. Conscious means nothing but the common peculiarity of these processes. Our question then is really: Where is perception (or thought or feeling), and what produces it? In the case of perception, our answer is definite. Perception is produced by the cooperative action of receptors, afferent and efferent neurons, brain cells and in many cases muscle cells. Perception is this total response, and hence it is located where the response is, namely all along the line, from beginning to end. Feeling could be as specifically localized, except that, unfortunately, we use the term freely in a double way: to designate the stimulus pattern, and also the response it initiates. Thus in popular language, hunger, pain and joy are feelings; but we speak also of feeling hunger, feeling pain and feeling joy. In the first sense, the feeling is the stimulus pattern to the visceral and somatic receptors, and is, spatially, in the soma and viscera. In the second sense, the feeling as a process of response is all along the line, from the stimulated tips of the receptors to and including the resultant action pattern.

Although I claim that any brain (aside from injury, degeneration and nutritional conditions) is in its infancy capable of development to the point of function possessed by even the best brains, I must admit that there are individual differences in capacity. Some persons simply can not be trained to the height of skill which others show, in music, in mathematics, in general intelligence. This statement of course is not absolute, but merely means that these persons can not be trained by the same

procedure as that by which others are trained, and perhaps by no procedure now possible. There is, theoretically, an environment in which the child of unmixed feebleminded stock may develop into a person of high intelligence, just as there is theoretically an environment in which the child of pure blue-eyed ancestry might develop brown eyes, but these environments may be incapable of actual installation without killing the individual.

If we grant individual differences in capacity under practicable environmental conditions, and yet claim that these differences are not inherent in the brain, we are forced to the only alternative, namely, that they are due to the periphery, that is to say, receptors, muscles and glands. There is nothing startlingly new in the ascription to the periphery of importance for mental development. The only novelty lies in the emphasis on which I insist. We have long known that blindness and deafness are definite obstacles to normal development of the mind. In the usual type of environment, the deaf child is very greatly retarded in mental development, although the results may not be of practical importance under the usual circumstances of civilized life. From an observation of anosmic individuals, I am convinced that their mental processes are even more different from those of normal persons than are those of the color-blind, especially in their emotional lives and the manifold factors dependent on the emotional responses.

These, however, are relatively minor determinants. The glandular and general metabolic characteristics I suspect of being major determinants. The muscular system, however, must not be underestimated. The muscles are not merely immediate agents in adaptation to environment: they furnish a continuous component in the stimulus pattern, and hence a constant factor in training. Not enough attention has been paid, heretofore, to the characteristic muscular development of different races. Even in the case of the true Negro, where the striking characteristics, aside from color and hair texture (which are probably negligible) and glandular peculiarities (which are not negligible), are in the length and form of leg and in the leg musculature, comparative studies of motor abilities have been made on the hand and arm action, but not on leg and foot performance.

Even height and weight are not to be ignored. Small men show traits which are different, on the average, from those of tall men, and the stature of the African pygmies is certainly not without its effect on their mental attitudes. The effects of stoutness and fatness on civilized mentalities may be complex in their production, but they are none the less real.

As for the differences in the chemistry of different individuals, and in the function of such peripheral mechanisms as the muscle plate in its relation to the muscle, where slight variations would be capable of producing large results, we have not even scratched the surface of the problems. The investigation of these peripheral factors I believe to constitute the most pressing problems of psychology.

Let me now illustrate my heretical theory more objectively. Suppose that a group of English babies from Lancashire were exchanged at birth with a group of babies from a Negro tribe such as the Wolof, or a similar fairly homogeneous breed. Suppose that the changelings were, in both cases, brought up by their foster parents with no prejudice on account of color or details of form and feature, and suppose them to be treated by neighbors, tribesmen and townsmen in the same impartial way, and to be exposed to the same training, the same education, the same social in-

fluences, as their foster brothers, and to eat the same types of food and wear the same types of clothing as their associates. In other words, suppose the babies from Africa to be brought up as nearly like English babies as their own constitutions would permit, and suppose the babies from England to be brought up as nearly like Africans as possible. What would be the result? We have enough data from actual transplantation of breeds to enable us to predict a large part.

We can be certain that the adopted children will be modified in many particulars towards the types of the foster breed. The babies from Africa will, when they grow up, differ from the English less than Africans brought up in Africa differ from English. They will speak English; they will have English interests, attitudes and manners; and in their thinking processes they will be somewhat, at least, like the English.

On the other hand, we know that in certain characteristics they will still be little, if any, modified from the type of the original breed. They will still be black; their hair will still be woolly, their lips thick, their legs long and their sweat glands peculiar. In almost all anatomical respects, and in some physiological respects, they are still Africans.

Apparently, then, we have two contrasting sets of personality traits. On the one hand, the physical, not affected by the environment, but determined entirely by heredity; on the other hand, the mental, very markedly determined by the environment, relatively little by heredity.

This statement would have been acceptable a decade or two ago, but to-day its very form carries its denial with it. To say that certain traits are determined by heredity and others determined by environment is to speak in terms of a mythical heredity and a mythical environment which have gone the way of the gods of Greece. We know of no differences in the force of heredity or the effectiveness of environment from one characteristic to another, physical or mental.

These principles stand out in modern theories of development.

First: every characteristic or trait or modification is as much a product of environment as is any other. In no case therefore can the effect of heredity be said to be greater or less than the effect of environment; and *vice versa*.

Second: differences in absolute measurements of traits dependent on differences in environment may be said to be great or small in relation to the environmental differences only in terms of the importance ascribed to the differences at the time; and this importance is a highly changeable matter. For example, a change of one per cent. in the shade of the skin (that is, in the coefficient of light absorption) may be said to be small, and a change from savage habits of eating and deportment to English manners may be called large. But this means that the one difference is such as happens at the moment to be of no great interest to us, while the other happens to affect our attitudes. Really, the two differences are incommensurable. Even if we should compare a difference of 5 per cent. in skin shade with a difference of 5 per cent. in length of leg, the two differences are still incommensurable and our practical evaluation on one as compared with the other is still subject to reversal as situations change.

Third: although we may admit a theoretically possible equivalence of environments, so that the effect of a certain environmental pattern may be the same as the effect of a certain different pattern, we expect actual equivalence to be attained very seldom. The same holds *mutatis mutandis* for hereditary patterns. We expect, therefore, in general, a difference in development where the

environment differs, and a difference in development where heredity differs. The resultant differences, however, may be great or small in respect to the units of measurement employed, and they may be important or unimportant from the pragmatic point of view.

All this bears on our expectation of results in the baby-swapping game. It is not probable that the skin color, the hair texture and form, the skeletal proportions and structures or any other physical characteristics are actually unaffected by the food, climate, types of activity enjoined and the other environmental influences in infancy and youth. Variations in these features may be practically negligible in the case of our swapped babies, but we can not assume that they are not present. On the other hand, in the traits which are said to be greatly modified by the environment—speech, manners and the mental processes generally—we have no warrant for supposing that our swapped babies become actually of the type of their foster folds, although in certain features the differences may be negligible.

In other words, our Africans have a certain inherited endowment, which is nothing fixed or absolute but which is merely the potentiality of developing in another, more or less different, way in another environment. The English environment has, in general, an effect which differs in an important way from the African. It shows strikingly in the mental traits. On the other hand, the Africans and the English have different potentialities of development in the same environment. It shows strikingly in the physical traits. But we have as yet no reason to suppose that both the results of differences of environmental effect and the results of difference of endowment do not apply throughout the total range. Our adopted babies from Africa will not be English, either physically or mentally, and our adopted babies from England will not be Africans, either physically or mentally.

Now let us take another supposition. Suppose that instead of swapping babies we could merely swap brains. We would have African babies with English brains brought up in the normal African way, and English babies with African brains brought up in the normal English way. Of course this is a little more drastic supposition than our first one, for we have at present no way of performing the operation; but the supposition is nevertheless useful. What would be the result of the brain interchange?

In accordance with the theory I have advanced, there would be no effect whatsoever, so far as the development, behavior and mentality of the individuals are concerned. The English babies with Wolof brains would develop into Englishmen indistinguishable, by any measurements, from ordinary Englishmen of the same type and environments. Wolofs with English brains would become thoroughly ordinary Wolofs. There is just one exception to be made. The transplanted brains, after maturity, might still retain, for theoretical absolute analyses, something of their original characteristics, if there really are any essential differences between Wolof and English brains in their early embryonic stages. But these hypothetical, vestigial differences would be devoid of consequences in any other parts of the organism or in the actual functioning of the brains themselves.

These inferences are based on the assumption of brain transfer at a very early age. I am not attempting to speculate as to the limiting age in the embryo or fetus. If now we should consider the effects of transplantation at a late age, the results would be very different.

The brain of an adult Wolof, if miraculously transplanted into the skull of an Englishman, would not serve the

Englishman at all adequately. Neither would the Englishman's brain serve the Wolof. The two brains have received radically different training and have undergone different development, and are hence differentiated in ways which very probably can not then be reversed. To what extent the adult brain could be retrained I am not able to guess. There is warrant, however, for the belief that the years of infancy and childhood are the years during which the foundations are laid for all further training.

It will be understood, of course, that the transplantations I am describing are put forward merely for the purpose of sharply outlined illustrations. Although it is by no means impossible that such transplantation might be made in the embryonic stages, nothing in my hypothesis depends on the possibility of such an operation.

Theorization of the sort involved in the hypothesis of brain superpotentiality may seem, at first glance, a futile form of amusement. I do not think it is. Quite aside from the physiology of the brain, the modern progress in psychology has been from dependence on assumed central factors determining mental development towards more intensive search for peripheral determinants. In this statement I am obliged to use two terms implying physiological reference. That is because our habits of making such reference are solidly fixed. The distinction can be stated in other words, but not so sharply. There is no doubt but that our purely psychological data and the view-point to which they have led us imply a cerebral theory such as I have outlined. For the critical evaluation of our psychology, it is necessary to recognize this clearly. We have left behind us, in America, the psychological constructions of Wundt, Titchener and Thorndike. Most text-books of psychology still talk of stimulus and response in archaic fashion, as if a spot of light falling upon the retina were the total stimulus resulting in an action limited to the contraction of the triceps muscles. But most text-books of psychology are epitomes of discarded theories and exploded facts, and the primary work with graduate students is to purge them of the rubbish they have learned in elementary courses.

For nearly a generation, the facts of integration have been recognized. We know that human animals respond, not to limited stimulus applications, but to patterns. We know that the important characteristics of the stimulus patterns are both spatial and temporal. While it is over-generalization to say that all the receptors participate in every reaction, that is the actual limit. Illustrations of the pattern characteristic are too simple and omnipresent to justify dwelling on them. Likewise, it is an over-generalization to say that all effectors participate in every response, but it is not much of an over-generalization.

The fact that patterns are the real stimuli has been recognized in certain special cases for centuries. It is emphatically indicated in Stratton's classic experiment to which I referred above. But this fact was not brought forward as a basic fact in all experiences until the last generation. For this delay two things were responsible. First, the survival of the doctrine of sensation: mental elements out of which whole perceptions were supposed to be compounded, and from copies of which, properly fitted together, ideas were formed. William James started the revolt in America against this synthetic doctrine, but the weight of two schools of psychology, clinging to established formulations, has kept many simple followers to the old faiths.

The difficulty of fitting the integration or pattern facts to the old brain theories was another strong deterrent to prog-

ress. Stimulus patterns as units of stimulation and action patterns as units of action require neural patterns as units intermediate between stimulus and action. This refuses to fit the old theory. Moreover, although sensory qualities may be analyzed out of the objects of perception, and localizing something called sensation in the brain may serve as a temporarily satisfying physiological scheme for them, relations are just as real analytic results in our perceived world, and where in the brain are their psychic substitutes to be localized?

The development of perception is clearly a matter of progressive integration, in which the responses are due to various stimulus patterns in which the entire substitution of visual for auditory, auditory for tactual and so on is permissible. Under the spell of the localization theory, this was forced into a childlike play-scheme of the aggregation of sensations and images, in which images were changed into sensations and *vice versa* as the explanation demanded. This could not last. The obvious need for psychology is to discover, recognize and formulate the laws and principles of perception, thinking and feeling. If the facts do not fit the theories of brain physiology, then the theories must be remodeled to fit the facts.

In Germany, the same process is under way, somewhat belatedly. Gestalt theory is a belated recognition of principles which have been accepted as matters of course by progressive American psychologists for twenty-five years. Like all belated movements, the Gestalt theory is very much confused, and suffers too at present from an exceptionally bad attack of *substantivitis:* the belief that when one has assigned to a group of phenomena a class name, like insight, one has thereby explained the phenomena—a disease by no means confined to Gestalt.

The Gestalt movement is without doubt helpful in stirring up the old bones, and even in America seems to be useful to the psychologists of the dissolving schools who had failed to take note of the progress seething just outside their scholastic walls. The modern views are in the way of universal adoption, and retrogressive movements such as behaviorism and psychoanalysis do not seem to have had any serious deterrent effect except in confusing the layman as to what is going on.

It is, however, just as important for brain physiology as for psychology to take stock of changing situations. There are two aspects of brain physiology: one, the chemical and electrical study of neural processes and their relations to the histological findings; the other, the interpretation of histological data in the light of psychological facts and theories. The first makes progress slowly, and the second constitutes 95 per cent. or more of what passes for brain physiology in text-books and lectures. The apparently rapid progress is most frequently into blind alleys, because the interpretations are based on outworn psychological theories or hastily assumed fact. Brain physiology, in the second or usual sense, is distinctly dependent upon psychology, hence the modern psychologist must actively interest himself in this field and not accept in simple faith the constructions based on the incomplete psychology of the past, or those of psychologically inexpert theorists of the present.

EARTHQUAKES, A CHALLENGE TO SCIENCE

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It is customary to think of the earthquake in terms of destruction and loss of life. The damage due to the earthquake is in some cases overshadowed by the damage due to fire which the earthquake has started and whose extinction is prevented by damage to the water system.

From the view-point of the seismologist the destruction caused by an earthquake is of secondary importance, whether it is the result of the shaking or of the fires. However, since the aim of science should be to benefit mankind and since the urge to scientific investigation comes in the first instance from the desire to reduce or prevent loss of life and property, it is proper to emphasize this feature of the earthquake problem.

In addition to the obvious possibilities of damage to cities and towns, there are a number of less obvious kinds of damage which have caused heavy loss in the United States in the past few years. These serve to illustrate the possible ramifications of earthquake investigation, now that most of the earth is either inhabited or put to the use of man.

One of these is the causing of landslides along railroad lines and highways through sparsely settled regions, a matter of importance on account of the large amount of mileage along the bottom or on the sides of steep slopes. This was best exemplified in the earthquake of June, 1925, which brought a great rock slide down on the tracks of the Chicago, Milwaukee and St. Paul Railroad in Montana (Fig. 1). This blocked a tunnel entrance for nearly a month and occasioned great expense for its removal. More serious loss was narrowly averted as one of the crack trains had pulled clear of the track covered by the slide less than a minute before.

The breaking of cables through submarine earthquakes has occurred in regions such as the Caribbean Sea, since cables were first laid. However, the only place in the North Atlantic Ocean where cable breaks from such causes might have been anticipated was the so-called Telegraph Plateau, a ridge in mid-Atlantic which extends almost from the Arctic to the Antarctic and where earthquakes have occurred and will probably continue to occur. However, seismologists were greatly surprised when on November 18 of last year there occurred a great earthquake in the vicinity of the Grand Banks of Newfoundland, which broke ten out of twenty-one cables crossing the area. Most of these were broken at more than one point, and while in a general way the principal breaks were within a circle about 125 miles in diameter which contains the epicenter of the earthquake, many of the breaks were far to the south. No adequate explanation of the wide-spread breaks is as yet available. They did not all occur at the time of the earthquake, but some of them were several hours later. The loss to the cable companies has been heavy in loss of tolls and in cost of repairs, which were exceptionally difficult in the winter season.

It is, of course, important to know whether or not the persistent reports of changes in sea bottom are correct. It is almost impossible to settle this question because of the inadequacy of earlier surveys. However, the vessels of the U. S. Coast Guard which are engaged in the International Ice Patrol are now taking acoustic soundings *en route* to

FIG. 1. ROCK SLIDE

DUE TO EARTHQUAKE, BLOCKING RAILROAD.

and from their sea stations to Nova Scotia, where they base during the iceberg season.

A rather rare event for North America was the destruction of life and property on the coast of Newfoundland by a tidal wave, which was most disastrous at Placentia Bay. There seems to have been an exceptional piling up of the wave owing to the peculiar form of the harbor entrance, since at Sable Island much nearer the place of the earthquake there was no report of a tidal wave. At most points in New England a heavy gale was raging and the seas made it impossible to distinguish earthquake effects. However, it was recorded on tide gauges on the outer coasts of New Jersey and Maryland (Fig. 2). It is interesting to note that the wave arrived earlier at more distant than at nearer points. Since the velocity of such a wave is fixed by the depth of water, it was possible to deduce a position for the origin of the wave which is not greatly different from the adopted epicenter of the earthquake.

Much the greater number of earthquakes occurring throughout the earth can be associated with mountain growth, remembering that many island groups simply represent the tops of submerged mountain chains, and also considering that the submerged ocean troughs which parallel mountain chains are in some way associated with them. Earthquakes of this type occur all around the rim of the Pacific Ocean and elsewhere.

There are, however, many earthquakes (and this is especially true in North America) which have no apparent connection with mountain building, and which have occurred in places where the surface geological conditions

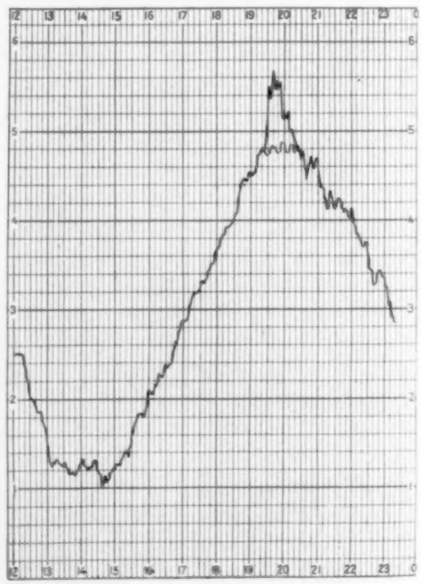


FIG. 2. TIDAL WAVE

FROM GRAND BANKS EARTHQUAKE OF NOVEMBER 18, 1929, RECORDED AT OCEAN CITY, MARYLAND. BROKEN LINE AT MAXIMUM INDICATES NORMAL TIDE.

give no clue to the reason for their occurrence.

One of the most interesting of these was the New Madrid earthquake of 1811, which centered near a place of that name on the banks of the Mississippi River in southeastern Missouri, which by itself was classed as one of the twenty greatest earthquakes in known history and which was followed by two other great shocks in 1812 which were almost of the same intensity, the activity continuing for nearly a year.

The shock was very severe and was felt sharply at Charleston, South Carolina, and Savannah, Georgia, but the feature of greatest importance was the formation of the so-called Sunken Country, or an area of 30,000 square miles which sank through five to fifteen feet, though limited areas throughout the region were raised a similar amount above the previous level. One of the results was the formation of Reelfoot Lake in Tennessee and changes in the drainage system of the St. Francis River. While the country was very sparsely settled and there were few people to report the changes, there is evidence of various sorts, as the drowning of cypress trees, to establish the principal facts. Earthquakes have been felt in this region probably every year from that time to this, and in several of the more severe shocks moderate changes in level over limited areas are reported to have occurred.

Equally difficult to explain is the earthquake at Charleston, South Carolina, in August, 1886. There were no changes of level, but the intensity was shown by the twisting of rails and the formation of sand craterlets. The principal interest in this earthquake from the view-point of the seismologist is that it occurred at all.

A relatively unimportant earthquake but one of considerable interest was that which occurred in the Panhandle of Texas in July, 1925. Here, again, inspection of the surface gave little indication of any reason for an earthquake, but drillings for oil brought out the fact that the region is the site of a buried mountain with steep slopes, a formation more likely to be associated with earthquakes than a flat or rolling plain.

It has been stated that submarine earthquakes are frequently associated with deep troughs. An excellent example is the trough paralleling the west coast of Mexico and Central America which was located and developed in 1924 by the steamer *Guide* of the Coast and Geodetic Survey by acoustic soundings (Fig. 3). With the development of interest in this subject which shortly afterwards resulted in assignment by law of seismological observations on the part of the government to that bureau, a special effort was made to find to what extent known earthquakes were located in this trough. A number were found, and since that time there have been earthquakes every year, including considerable seismic activity.

These different types of earthquakes serve the purpose of illustration. Earthquakes present many challenges to science. Some of these have been met;

FIG. 3. SUBMARINE TROUGH AND ASSOCIATED EARTHQUAKES.

others have not been met at all, but in most cases the condition is intermediate between these extremes.

The first challenge is the cause of earthquakes. This is intimately related to the history of the earth, and if we had the complete answer we would have the key to much that is obscure in that history. For many years earthquakes and volcanoes were closely associated in the public mind. We now know that, while they are related, and all volcanic outbreaks are accompanied by earthquakes, all the greater earthquakes occur in regions remote from volcanoes or if in volcanic regions at a time when the volcanoes are not active. In the great Japanese earthquake of 1923 Fujiyama, an extinct volcano, was strongly shaken but was not roused to activity. On the other hand, the greatest eruption of Vesuvius in recent times was an explosive outburst near the same time as the California earthquake of April, 1906. This may have been a coincidence, but there may have been some relation, though if so it was more probably in the trigger force which timed the events than in the events themselves. It would seem that the volcano is a localized and rather superficial phenomenon as compared to the earthquake.

In general, earthquakes are due to slipping of two rock surfaces on one another, and it is peculiarly helpful to the study when this effect reaches to the surface. Examples of this are the California earthquake of 1906, where horizontal slipping reached as much as twenty-three feet; the Yakutat Bay, Alaska, earthquake of 1899, where vertical slipping was the greatest ever recorded on land in historic times, reaching a maximum of forty-eight feet; the 1929 earthquake in New Zealand with vertical slipping of twenty feet, and a number of Japanese earthquakes, notably the Mino-Owari earthquake of 1891, in which case the slipping was on a diagonal plane. I do not know whether we may assume that the amount of the slipping at the surface is the same as at the origin but there is certainly a very direct relation. The observed conditions in California led Harry Fielding Reid, of Johns Hopkins University, to propound his elastic rebound theory which is briefly as follows.

Let us assume that there is a plane of weakness and one side remains stationary while there is movement on the other. As soon as the strength of the rocks is exceeded there will be slipping. However, there is too much friction and irregularity to permit this being accomplished at once and there are a series of slips of irregular character which in turn send out the earthquake waves and account for their irregularity.

Now in order to study surface effects we must have accurate determination of the positions and elevations of fixed points in order to know the changes accompanying an earthquake and also to find out whether or not there is slow creep of crust between earthquakes. The Carnegie Institution of Washington, which through its advisory committee of seismology, Dr. Arthur L. Day, chairman, is the chief coordinating agency and strong financial supporter of the program of earthquake investigation in California, is convinced that this is a major part of the program. Accordingly through a special appropriation by Congress the Coast and Geodetic Survey has accomplished or will in the near future have accomplished the following things. It has executed precise triangulation in California and has in so far as possible connected it with earlier work, especially that done after the earthquake of 1906, so that it has the greatest possible value in determining changes; it has connected this work at three places with the more stable seismic regions to the east and in this connection has readjusted the triangulation of the western half of the country, using a method de-

veloped by Dr. William Bowie, chief of the division of geodesy, which reduces to a minimum the effect of the unavoidable errors on the final results; it has determined the position of a large number of monuments in a selected region so that the movements at different parts of the area can be studied. A similar program has been followed with regard to precise levels. The plan includes repetition of both classes of work at suitable intervals in order to record the changes.

No precise triangulation or leveling suitable to the purpose has been done in the New Madrid region. We know so little of the process which brought about the lowering of the surface over 30,000 square miles that we can not say definitely that it was the result of shaking down of loosely consolidated materials, though this is the generally accepted explanation. If this is adopted, is it likely that movements at the origin have any chance of being reproduced even approximately at the surface? These questions must remain unsettled until precise triangulation and leveling can be undertaken over this region. Owing to the great area involved, detailed investigation would be a major task. However, until this can be done an essential part of the fundamental data will be lacking.

It seems likely that we will have to get our information as to what occurs at the origin through the seismograph, and it is by no means certain that the possibilities in this direction have been exhausted. Frank Neumann, of the Coast and Geodetic Survey, has deduced the direction of the initial thrust from the records and has obtained the same azimuth from records of six stations in as many different directions from the epicenter. Also in the case of the California earthquake he obtained an azimuth almost that of the San Andreas Fault along which the slipping occurred. Perry Byerly, of the University of California, makes use of several phases and has deduced with even more detail the direction for the Chilean earthquake of slipping in a diagonal plane which agrees remarkably well with that deduced by Bailey Willis from inspection of this region.

An important problem in regard to which we have little satisfactory information is the course of events leading up to an earthquake. The statement is sometimes made that an earthquake occurred "out of a clear sky" without any previous activity in the region. My investigations in the preparation of an earthquake history of the United States a few years ago convinced me that this never occurs. In the case of the New Madrid earthquake, Indian tradition and other evidence support the view that there was another earthquake of perhaps comparable severity not more than one hundred years before. At Charleston, South Carolina, there was an earthquake in 1857, twenty-nine years before the great one. Records show that considerable activity preceded the California earthquake of 1906. However, it is still impossible to distinguish moderate earthquake activity from that resulting in a great shock. In the Imperial Valley there have been a long series of moderate earthquakes which have died away and at other times a much shorter series which resulted in an earthquake of great intensity. In February and March of this year there was an occurrence of this sort with considerable damage at Brawley.

It is, therefore, of great importance that the Seismological Laboratory of the Carnegie Institution of Washington and the California Institute of Technology at Pasadena is, with its subsidiary stations, locating all earthquakes and wherever possible associating them with the geological formations. Eventually this will lead to a definite history of the events leading up to an earthquake which, though it may not apply to other earth-

quakes, will at least throw light on the problem. A similar program is being developed in the San Francisco Bay region, through the universities located there and cooperators.

It has been stated that earthquake epicenters are being associated with geological formations. Since the seismometer is capable merely of measuring a time interval between the arrival of phases, it is necessary to assume the velocity of transmission, obtained partly from assumption as to place of origin and partly on the theory of transmission. If the path of the wave is chiefly far beneath the surface, the difficulty is less, since though there are changes in elasticity and density the conditions are much the same for all parts of the earth, so that the velocity will be much the same for the same distance between earthquake and recording station for any part of the earth.

However, when the station is within two hundred miles of the point of origin of the earthquake there are likely to be appreciable departures of the velocities from the average values because the waves pass through different geological formations and the velocity may differ with the direction. One method of attacking this problem is to measure the time of travel from large explosions, since in this case we know the point of origin accurately, and important earthquake wave theory has been developed and confirmed in this way. Earthquakes may be and are used, but it is always difficult to know the exact point of origin.

Seismic prospecting is a practical application of this method, though in this case we are associating changes in velocity or reflections with the geological formations for the purpose of identifying the latter and following them beneath the surface.

Permanent changes of the surface have been discussed. The Japanese have recently found that a definite tilting of the ground precedes by a few hours the occurrence of a strong earthquake, and by means of a tiltometer have proved such an occurrence for several earthquakes. There is no certainty that a similar effect would be observed in this country, since in Japan there appears to be a tilting of blocks, while such a formation either does not exist or is not a major factor in this country, but John R. Freeman has performed a real service by bringing one of the instruments from Japan and lending it to Stanford University in order to test the matter.

The principal earthquake waves as shown in Fig. 4 are P, S and L. P waves are longitudinal; that is, the vibration is in the direction of progress, and they take the path indicated between the earthquake and the recording station. The S waves are transverse; that is, vibrations are normal to the direction of progress, and the S wave follows the same path as the P but at a lower velocity. L waves travel along the surface. Many different waves result from reflection and refraction of P and S. The P and S phases and their reflections are seen in many seismograms, but for nearby earthquakes we often find two P's and two S's, a fact which must be explained. The first, that advanced by Mohorovicic, calls for a reflecting layer sixty kilometers below the surface which he called the continental layer because the observations demanded less thickness beneath most of the oceans and practically none beneath the Pacific. Harold Jeffreys (Fig. 5) has developed a theory which is in agreement with the probable wave transmission characteristics of the layers affected. He has layers of discontinuity at ten and thirty kilometers depth respectively with different physical conditions for each layer and for the region below the lower layer.

The next fact to be fitted into the pic-

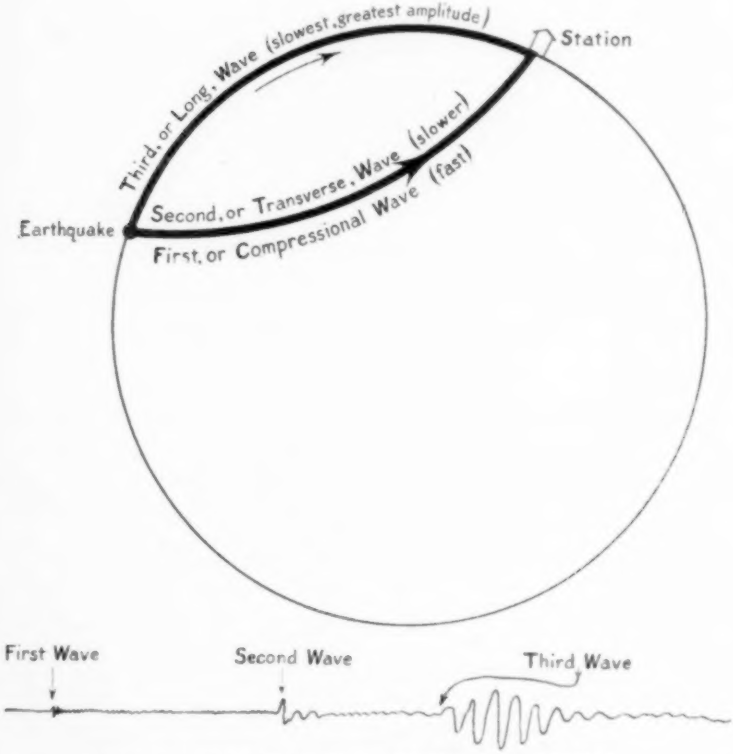


FIG. 4. DIFFERENCE IN TIMES OF ARRIVAL
OF FIRST AND SECOND WAVES GIVES DISTANCE FROM EARTHQUAKE TO STATION.

ture is that of isostatic compensation (Fig. 6). This requires that if the earth's crust were divided into columns one hundred miles square and about sixty miles in depth, they would weigh the same even if in one case one column had part of a mountain range at the surface and the other had its surface far beneath the sea. This conception is derived from study of precise gravity determinations and is the only one which makes it possible to fit together gravity observations made at different places. It is confirmed by determinations of average density. Now the fact of equilibrium is surprising in itself,

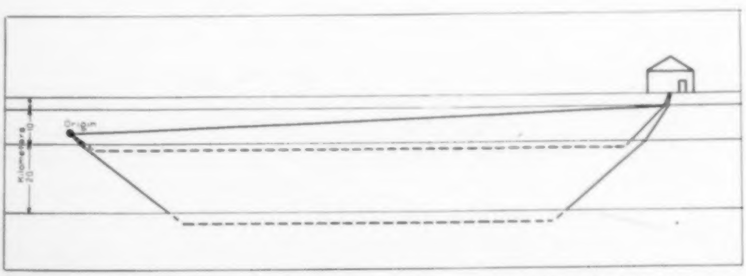


FIG. 5. PATHS OF PRELIMINARY EARTHQUAKE WAVES
THROUGH UPPER LAYERS OF EARTH'S CRUST, ACCORDING TO JEFFREYS.

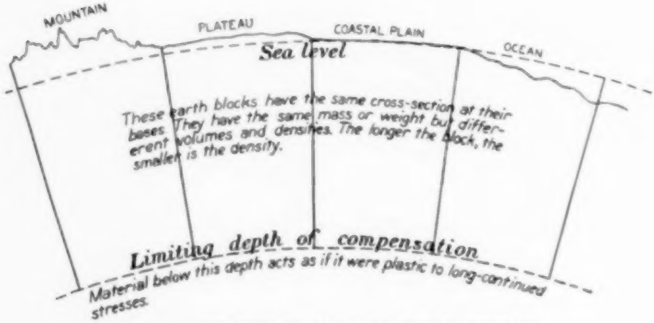


FIG. 6. SECTION OF EARTH'S CRUST
ILLUSTRATING THEORY OF ISOSTASY.

but it is still more so when we consider that it has been maintained in spite of a constant exchange from mountain to lower levels of vast amounts of material by erosion and sedimentation. This implies adjustment and, in general, this must go on mainly near the base of the column where the material is relatively plastic or will respond to long-continued stress without rupture. It is quite conceivable that stresses in the upper part of the column may build up and have to be relieved in such a way as to cause earthquakes. This is not set forth as a complete explanation of earthquakes, and all that we are justified in maintaining is that all earthquakes should have their origin above the depth of compensation. Occasionally it is held that one type of earthquake has a deeper origin, but for the vast majority it is certain that their depth is very much less.

In order to have any conception of earthquake causes, it is necessary to know at what depth they occur. Unfortunately the information is very defective, since only in some parts of Europe are conditions such that accurate determinations can be made. There must be a number of stations with suitable instruments within a few hundred miles of the earthquake; there must be instruments for recording the vertical component, and the time must be accurate to the tenth of a second. For a large number of European earthquakes the depth has been found to range from forty-five to sixty kilometers, though there undoubtedly are a very great many at less depth and some of the smaller ones may be very near the surface. I do not feel that there is as yet sufficient information on which to base a discussion of the place of earthquake origin in relation to the various layers that have been mentioned.

Now if the origin is at these considerable depths it is difficult to see how the so-called "trigger forces" act. However, it is practically certain that in the case of many earthquakes a state of unstable equilibrium continues for a long time until some sudden action of a very much smaller force than those which are involved in the earthquake itself brings about the occurrence. Such forces may be due to an exceptionally high tide on the coast or to the sudden melting of a large mass of snow but more generally to sudden change in barometric pressure. The great Japanese earthquake of 1923 was accompanied by a severe typhoon which may have fixed the time of its occurrence.

While we are considering the upper layers of the earth's crust there is another form of wave action which is important. The so-called long waves pass through the layers near the surface and

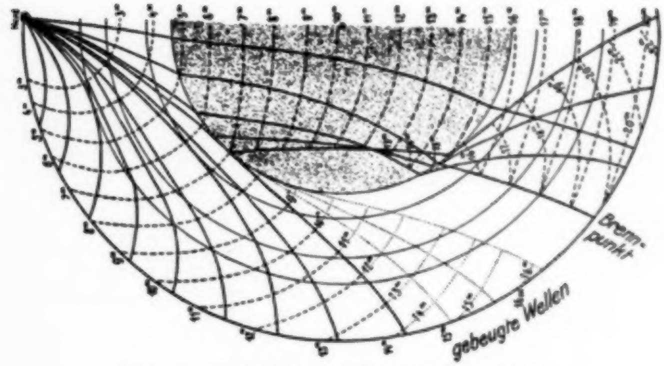


FIG. 7. EARTHQUAKE WAVE PATHS
THROUGH THE INTERIOR OF THE EARTH, ACCORDING TO GUTENBERG.

usually are of such long period that they extend sufficiently far below the surface so that local variations of geological conditions have little or no effect. Their velocity, however, responds to the average conditions of the layer through which they travel. It is, therefore, of interest to note that the waves speed up 18 to 20 per cent. beneath the Pacific Ocean as compared to continental paths, and this can be explained only on the assumption of greater elasticity which in the case of rock is associated with higher density. This confirms in a qualitative manner the conclusions drawn from the theory of isostasy. The question as to whether the other ocean beds have the same velocity as the Pacific or an intermediate value is not yet finally settled, though the proponents of the theory that the moon was derived from the Pacific basin and of the Wegener hypothesis of continental drift generally assume that the values are intermediate.

Now going deeper into the earth there is much to be learned from the behavior of seismic waves, but I will merely point out the evidence for the generally accepted view that the central portion of the earth is in a condition more rigid than steel and yet liquid in so far as transmission of waves is concerned (Fig. 7). The boundary is sharply defined and is about 2,900 kilometers beneath the surface. The discovery of the core was due to the sharp changes that take place both in the travel times of waves and in their characteristics when the point is reached, and passed, where the waves graze the core. The other evidence is that the recorded phases from distant earthquakes can be explained only on the assumption that transverse or S waves do not pass through the core. The situation is actually somewhat complex, as both P and S waves reach the boundary and each separates into P and S waves. Only the P waves pass through the core. When these P waves pass through the core and again reach the boundary each is broken up into P and S waves and may reach the surface in either form. The condition of the core is deduced in part from the velocities which must be adopted to fit the observed conditions.

In presenting this general picture it has been necessary to omit a number of subjects which are of interest chiefly to the seismologist. With the principal problems and accomplishments set forth the next thing is to see what are the specific problems in the United States and what is being done about them. The present is a particularly suitable time for such a review because it is little

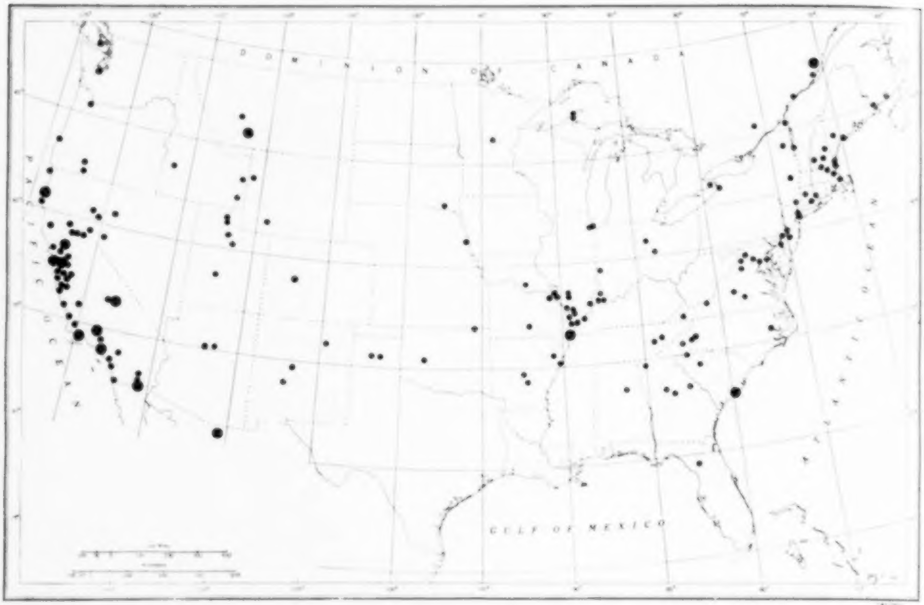


FIG. 8. KNOWN EARTHQUAKES OF THE UNITED STATES

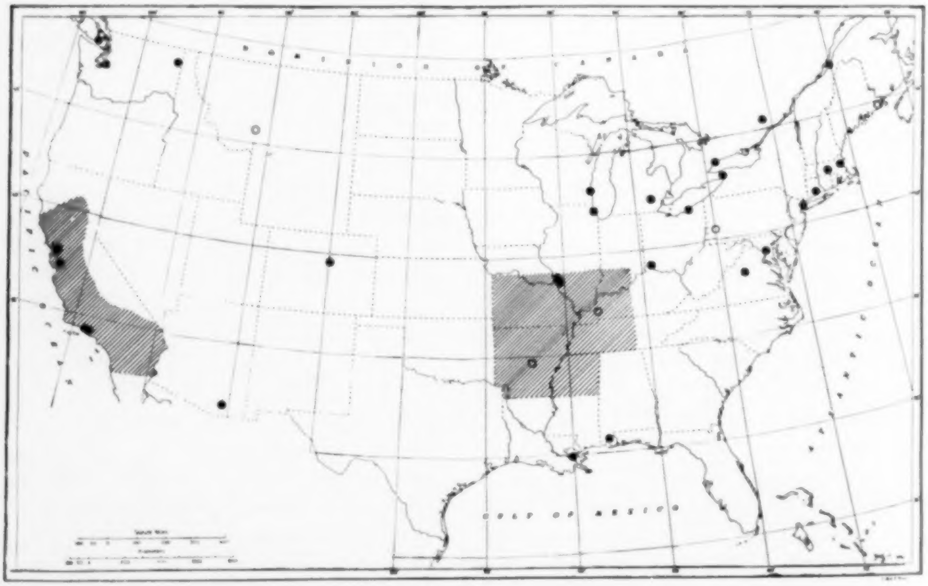


FIG. 9. SEISMOLOGICAL STATIONS

OF THE UNITED STATES AND ADJACENT CANADA. CROSS-HATCHING INDICATES AREAS OF SPECIAL INVESTIGATIONS OF SEISMIC REGIONS.

FIG. 10. WOOD-ANDERSON TORSION SEISMOMETER

more than five years since wide-spread interest and activity developed, and the next five years are going to be of great importance in establishing this work in its proper place.

The map (Fig. 8) shows the distribution of all earthquakes strong enough to be felt from the earliest times to the present. While principal activity is confined to definite regions, especially the New England region and adjacent Canada, the central region and the Pacific Coast region, there has been activity in nearly every state, and any institution which contemplates the installation of a station can be assured that sooner or later there will be recorded an earthquake at no great distance.

There are now or soon will be in the United States, including those new stations where the instruments are actually under construction and are recognized as first class, fourteen stations for recording earthquakes (Fig. 9). These are operated by the federal government, the universities and colleges belonging to the Jesuit Seismological Association and by other universities. There are other institutions where the instruments were installed some time ago and while useful are no longer considered first class. However, there are definite prospects of establishment of additional stations, particularly in the region where few or none now exist.

We no longer have to send to Europe for instruments to record earthquakes, though many fine types of instruments have been developed there. The first distinctly American seismometer was the Wood-Anderson (Fig. 10), developed in California. It is now unquestionably the best instrument for recording nearby earthquakes, and important contributions are to be expected from its records. This instrument responds to the extremely short period waves recorded by nearby earthquakes. Frank Wenner, of the U. S. Bureau of Standards, has developed another type of instrument better suited to the more distant earthquakes (Figs. 11 and 12). In this instrument the steady mass carries a coil; the motion of the support with regard to the coil causes it to move in a strong magnetic field, and the currents set up are recorded by means of a galvanometer. This general idea was developed by Prince Galitzin, of Russia, but the Wenner instrument has a considerable number of advantages. Another instrument is the McComb-Romberg, developed by H. E. McComb, of the Coast and Geodetic Survey, which includes a device modified from one developed by Arnold Romberg, of the University of Texas, for preventing effects of slow tilting of the

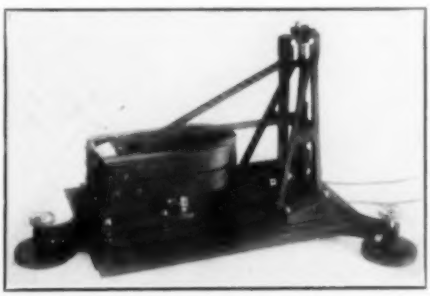


FIG. 11. WENNER SEISMOMETER

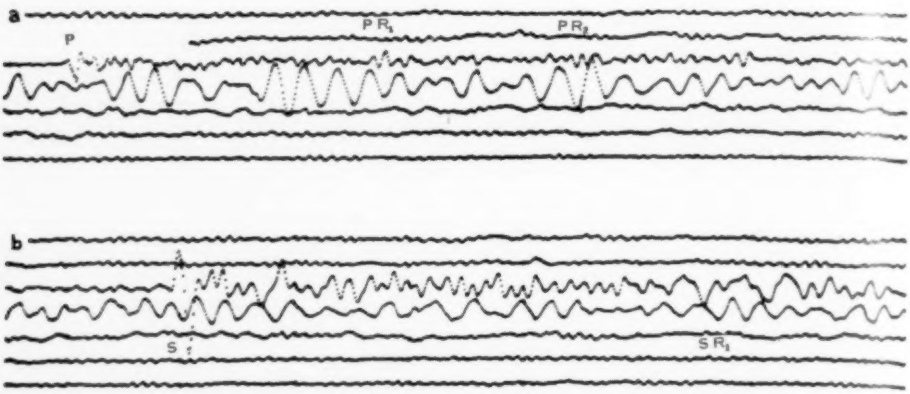


FIG. 12. RECORD PRODUCED BY WENNER SEISMOMETER

ground from affecting the records. This instrument, while a modern seismometer, has smaller magnification than those that have been described and is, therefore, well adapted to recording strong shocks and also to recording earthquake phases more clearly than sensitive instruments when microseisms are severe.

One justifiable European criticism of American stations is the lack of instruments for recording the vertical component. Since earthquakes are a three-dimensional phenomenon, recording in two dimensions is obviously incomplete. This is being rapidly remedied at many of the stations, especially at Jesuit institutions, and such installations will probably be made general within a few years.

The stations described are for the recording of distant earthquakes. There are important installations for recording of nearby earthquakes in southern California, in the San Francisco Bay region under auspices that have been mentioned and in the Mississippi Valley region under the auspices of St. Louis University and the National Research Council.

The interpretation of records is quite as important as securing them, and there are fewer persons who are expert in interpretation than in the securing of them. Interpretation with a large number of records available for examination is particularly useful. As yet the only places where such work is done are Washington, D. C.; St. Louis, Missouri, and Pasadena and Berkeley, California.

Now instruments alone can not tell the full story of the central region where an earthquake is felt. For this reason the reports from individual observers are important. While all reports are welcome those from careful and skilled observers are much better, and there is a rather elaborate organization which covers the United States and Alaska. The organizations include the Coast and Geodetic Survey, the National Research Council through its Divisions of Geology and Geography, the Jesuit Seismological Association, numerous universities, state geologists and others. It is customary to outline the area affected by an earthquake by so-called isoseismal lines or lines of equal intensity. There are many complications including that of proper intensity scale into which I can not enter, but in order to have these reasonably correct we must have a large number of records to eliminate uncertainties.

When severe earthquakes occur in any part of the earth the news is of interest and it is often very desirable to know at once where they occurred. For example, several years ago there occurred a great earthquake in central China and

through instrumental means this was known months before the arrival of any news. The position of the Grand Banks earthquake of November 18, 1929, which occurred at 4:00 P. M., was known by 9:00 P. M., though the position was beneath the sea, and later study did not change the position from the general region. This is accomplished by an organization which is now almost worldwide, of which an important share is due to American initiative and organization. The information is sent in code from seismological stations in many parts of the United States, Canada, Pacific islands and the west coast of the Pacific Ocean. From these the positions of the epicenters are determined at the office of the Coast and Geodetic Survey at Washington, redetermined at St. Louis University, and the combined results are then sent out to all who have cooperated. Science Service has an important place in the plan, and bears most of the expense. In addition, information is sent to Europe the night following the occurrence by addition to the weather message sent by the Weather Bureau through Arlington. This is rebroadcasted by Eiffel Tower. In addition to news value, seismologists find the information useful in the interpretation of their records.

Work is being done on all the problems that I have outlined, but I do not wish to close without making it clear that the engineering and insurance problems are not being overlooked. The American Society of Civil Engineers has had a committee which has collected information regarding types of buildings and structures which have withstood earthquakes. Building codes have been improved and design of large buildings has been given special consideration. Recently large dams and bridges in regions subject to earthquakes have been designed to resist earthquakes. Leland Stanford University is operating a large shaking table sixteen by twenty feet, on which large models of structures can be tested. Its motions can be made to simulate an earthquake, and all that is needed to make results of very great value is adequate funds for preparation and test of structures. Structural design is receiving much attention at the California Institute of Technology. Several engineers of prominence who attended the World Engineering Congress at Tokyo are greatly concerned in the expansion of the engineering phases of the earthquake problem after seeing what is being accomplished along these lines in Japan.

Insurance problems have received a good deal of attention from the National Board of Fire Underwriters and other organizations. It is a difficult problem to fix rates which will not be prohibitive and which will yet protect the companies against a possible major disaster. Organization of cities to deal with a major disaster has also received consideration.

Many things remain to be done, but it is evident that after a period of comparative neglect scientific men in the United States are awake to the challenge presented by the earthquake. Not yet is the effort comparable to that which has been made to solve the problems of fundamental physics or of astronomy, nor is the engineering attack organized on the basis of the important investigations being conducted under the auspices of the great engineering societies. Especially are there lacking the numbers of students upon whom the future of the work must depend for effective progress. However, the program is unfolding, and we are no longer dependent on a severe earthquake to arouse interest to the point of action.

MAPS FOR AVIATORS

By RAYMOND L. ROSS

CHIEF, AIRWAY MAPPING SECTION, U. S. COAST AND GEODETIC SURVEY

WITH the remarkable progress in civil aeronautics, the necessity for federal maps, compiled for the pilot in a rapidly moving plane, soon became apparent. Congress provided funds and authority in 1926 aggressively to aid and encourage the development of commercial aviation, and the task of making the necessary maps was logically delegated by the Honorable Herbert Hoover, then Secretary of Commerce, to the U. S. Coast and Geodetic Survey, with its trained personnel and a modern map-making plant, able to turn out additional work with a minimum increase in overhead.

In the organization of the Airway Mapping Section there is an unusual interlocking of bureaus. Quartered in the building occupied by the Coast Survey as a unit of the Division of Charts, it is also under the direction of the chief of the Aeronautic Branch of the Department of Commerce, from which funds for its maintenance are received. The arrangement is comparable to the organization of the Airways Division of the Bureau of Lighthouses, in its relation to the Aeronautics Branch, having to do with the lighting of airways, the selection of intermediate fields, establishment of radio ranges, etc.

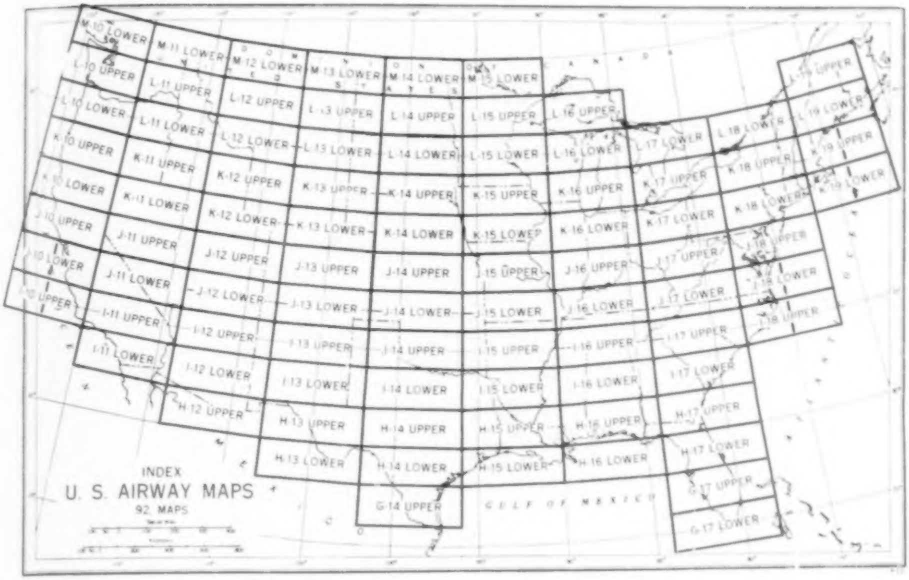
At present these maps are produced from information gathered from a great variety of sources, such as maps of the Geological Survey and General Land Office of the Department of the Interior; Forest Service, Bureau of Chemistry and Soils and Bureau of Public Roads, of the Department of Agriculture; post route maps of the Post Office Department, and state, county, railroad, automobile and other maps of commercial producers, as well as blueprints of various power and light companies. No one of the maps used shows all the features needed. Data concerning intermediate fields and beacon sites are supplied by the Airways Division of the Bureau of Lighthouses, while the positions of airports and auxiliary fields are obtained from the Aeronautics Branch.

The first airway strip map was issued on June 27, 1927, and at this time a total of twenty have been published with three others nearing completion, namely:

102 Dallas—Oklahoma City
103 Oklahoma City—Wichita
104 Wichita—Kansas City
105 Kansas City—Moline
110 St. Louis—Chicago
111 Chicago—Milwaukee
112 Milwaukee—St. Paul
114 Cincinnati—Chicago
115 Louisville—Cincinnati
119 Buffalo—Albany
127 Birmingham—Atlanta
128 Atlanta—Greensboro
129 Greensboro—Richmond
130 Richmond—Washington
131 Pueblo—Cheyenne
132 Los Angeles—Las Vegas
133 Las Vegas—Milford
134 Milford—Salt Lake City
135 Salt Lake City—Boise
136 Boise—Pasco

These, together with the fifty-two airway maps also published by the Army Air Corps, are sold by the Coast and Geodetic Survey at thirty-five cents each, with a reduction of ten cents per copy on all orders for twenty or more.

They are to the scale of 1:500,000, or about eight miles to the inch, covering



bands of topography along established airways of about eighty miles in width and averaging 250 miles in length.

They are specialized for the use of the aviator in a rapidly moving plane, who must orient himself promptly and has no time to study complex details. They emphasize the relative positions of outstanding topographic features as he sees them, with a limited number of standardized and easily understood symbols descriptive of others. By such symbols he can readily identify Army, Navy or Marine Corps fields, commercial or municipal fields, intermediate fields of the Department of Commerce, marked auxiliary fields, flashing and revolving beacons and other lighting facilities, transmission lines, one track and two or more track railroads, electric railroads and main and secondary highways as well as the towns.

With a map tentatively compiled, a trained engineer makes comprehensive check flights over the entire territory, after which necessary corrections are made and new features added. It is then ready for reproduction and printing.

Symbols for the larger towns are in yellow, with their names emphasized by capital letters. Smaller cities are indicated by uncolored circles and their names given in small type for possible usefulness in emergency landings.

The altimeter carried by the aviator indicates his elevation above sea-level and not the distance separating him from the ground over which he is passing, making information of the altitude and contour of the terrain of vital importance. This is shown by contour lines for every 500 feet, with their elevation above sea-level indicated. High points are also shown, so the pilot may fly at an altitude sufficient to clear each hill and mountain range and, when desired, use the passages between.

Green is used to indicate the lower altitude and brown the higher. The deepest green shade shows from sea-level to 1,000 feet, while the deepest shade of brown is used to depict the highest altitude, namely, from 9,000

feet to the maximum elevation included within the limits of the map. A lighter green is used to show from 1,000 to 2,000 feet, while brown tints are utilized to indicate elevations above 3,000 feet, the brown color being shaded progressively darker for 5,000, 7,000 and 9,000 and high altitudes.

The compilation of what will be known as sectional airway maps—a new undertaking—has now been started, since it has developed that over 75 per cent. of the annual flight mileage is away from the regular commercial airways. They will gradually replace the airway strip maps. The indexing system will harmonize with the International Map of the World, but the scale of the sectional airway maps will remain the same as that of the strip airway maps. In a general way, the first ones will cover areas where the strip map program is complicated by their crossing and overlapping in following the commercial airways. A total of ninety-two section maps will be required to embrace the United States, although it is possible that this number may be somewhat reduced by an alteration of the program along the Atlantic and Pacific coasts.

A RISE DOWN THE CANYON

By Professor ELLIS W. SHULER

SOUTHERN METHODIST UNIVERSITY

EARTH sculpture, in the semi-desert mountains of western Texas, is always puzzling even to the trained geologist. There is everywhere evidence of the erosive power of water, yet seldom is water to be found. The deep canyons are evidently cut by stream action, but there is to be seen not even the smallest stream to do this work.

The explanation is the thunder-shower and the cloudburst. This agency, since the dry soil is not covered over thickly with vegetation, may produce incredible results even in a single storm. Down the dry canyon sweeps a wall of roaring, frightful flood; then all is quiet and dry for weeks, perhaps months of time. In the brief period of flood more is accomplished than in years of quiet flow with normal rainfall.

Such a rise was seen by the writer last summer, and what is more unusual, he was able to record it in photograph.

"A seven-foot rise down Limpia," announced the hotel clerk to the guests on the porch at Fort Davis.

Early in the afternoon a thunderstorm had raged high up in the Davis Mountains, although no rain had fallen at Fort Davis.

Mr. Henderson had just told the new guest how he and a party of friends had been caught in such a rise the week previous. They had entered the stream thinking they could cross before the water drowned their car, but when the engine was killed they had to abandon the car hastily and wade to the bank. One woman had fallen into the current in her haste, to the great peril of her life. Then, hemmed in by two fords and a steep cliff, the party had spent the night around a camp-fire in the rain while the car was carried down stream.

Below the old fort there is a crossing which in previous years has taken more than one life. About an hour after the phone call the cavalcade of

DAVIS MOUNTAINS SOUTH OF FORT DAVIS

INDIAN SHELTER ROCK ON POINT OF ROCK WEST OF FORT DAVIS

hotel guests motored to this crossing. (Future tourists may be interested to know that a newly located state highway and bridges will make the canyon safe and even more interesting than formerly.)

Runners had been sent to warn campers, for despite repeated warnings, the shade of the cottonwood trees and the grassy bottoms along the canyons prove at times irresistible to the transient motorist. Natives of the region, too, who have seen the phenomena over and over again are never quite convinced that they can not "beat the rise," most often to their sorrow.

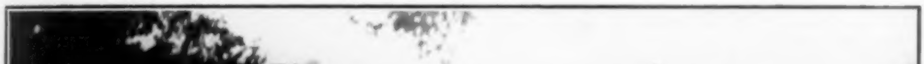
When the cavalcade arrived, the road crossing, a dip, was dry. In making a dip the bed of the stream is covered with concrete to prevent scouring. Posts are erected at the crossing with markers which show the depth of water running over the concrete slab and thus indicate the danger of the crossing.

It was 5.30 P. M., and the sun was shining. There was nothing to indicate the near presence of a raging torrent, and a little boy insisted that he be allowed to cross to the other side.

Presently in the distance came a murmur of waters, not alarming, but insistent. Then came a hissing, black, roily tongue of water down beneath the cottonwoods. It was not a wall of water, but a live, hungry tongue, black, dirty and covered over with bits of bark. It was a filthy and a fearful thing to see as it ran eagerly ahead of the main water mass.

Then followed the rising flood, filling the canyon bottom. Downward, tumbling and frothing, now arching up in the middle, then leaping in great bounds, thus the yellow tiger waters rushed down the canyon and over the road

ROAD DIP BEFORE THE "RISE" AT 5:28 P. M.



FIRST THE SNAKE-LIKE TONGUE AT 5:31 P. M.

THE FLOOD IS COME, 5:34 P. M.

AN ANGRY TORRENT, 5:37 P. M.

crossing with a mighty roar and pounding of rocks as the waters dragged them along the canyon bottom or beat them over the concrete slab.

It was a fearful sight; yet there was one thing which amazed. In the quiet of the sunset not a breath of air stirred. In the center of the flood stood three cottonwood trees with not over twelve-inch trunks. The waters swirled around them and beat upon them with savage blows, five or six feet high on the trunks, yet not a rustle or whisper came from the leaves on the trees!

It was exceedingly difficult to estimate the speed of the current because of the crooks and turns of the canyon floor; but from the time of the phone call it had taken the rise two hours to come an air line distance of about twelve miles. Three hours later, the road and dip were dry.

The visitor and tourist who see such a rise will know the explanation for the vivid water-searred canyons, and he will take care to place his camp high enough along the canyon wall so that he will not be overwhelmed in such a flood.

FOSSIL HUNTING IN THE KARROO, SOUTH AFRICA

By Dr. ALFRED S. ROMER

WALKER MUSEUM, UNIVERSITY OF CHICAGO

South Africa is famous for the wealth which its rocks produce. The greatest portion of the world's gold is derived from the Rand; its mines and diggings have almost a monopoly of the world's supply of diamonds. But the paleontologist, the student of fossils, is not interested (theoretically, at least) in gold or in diamonds, and when he thinks of the treasures of South Africa's earth, it is another type of wealth which is pictured in his mind. Covering half of the extent of the union is a great series of rocks, the Karroo Series, consisting of thousands upon thousands of feet of shales and sandstones and mudstones which, if their total array could be seen at one place would reach nearly six miles in thickness. In these rocks there is no gold, and in most portions no diamonds. But in them are found fossil riches—the remains of many kinds of fossil reptiles which peopled the earth nearly two hundred millions of years ago. It is of a fossil collecting trip to these beds by Paul C. Miller, Walker Museum's veteran collector, and myself, that I wish to tell.

But it is perhaps necessary to explain why we had wandered so far afield, for no other continent has such a great range of fossiliferous rocks as has North America. Our own deposits contain vertebrate remains of almost every period from the remote Age of Fishes to the Ice Age just behind us.

In this series of fossiliferous deposits, however, there is one great gap. The major part of the Permian and Triassic periods is represented by vast thicknesses of strata, but these rocks are almost barren of vertebrate life. This is particularly unfortunate, for these were times when crucial developments were occurring in the evolution of land animals.

In the Coal Measures occur our first records of the amphibians, the earliest vertebrates to adventure out onto the land. Toward the close of this period there first appeared the reptiles, from which all higher vertebrates have developed, and in Texas the "red beds," of slightly later (Lower Permian) age, have a wealth of primitive reptilian forms.

But with this our American record closes for a time. Despite repeated search, not a fragment of bone has ever been discovered in our abundant rocks of later Permian age. Still higher are more red beds, of Triassic age, in which vertebrates are still extremely rare. When we next find a good series of fossil-bearing strata we have reached the Jurassic, the heart of the Age of Reptiles, and all the varied reptilian groups that make this age so interesting are there on the scene. Dinosaurs of all sorts, flying reptiles, water reptiles, even the first birds and the early mammals have all made their appearance. The transitional stages, by which the primitive sluggish reptiles were transformed into these varied types, are for the most part not to be found in North America, and in Europe conditions are little better.

But in South Africa a different situation exists. The Karroo Series is composed of rocks of Permian and Triassic age. The lowest part, corresponding to

the fossiliferous beds of North America, is almost completely barren. Just at the point, however, where the story stops in this country it is taken up in South Africa, and through thousands of feet of rock thickness there are found plentiful remains of the animals of the middle and upper portions of the Permian and the greater part of the Triassic. Some of these are relicts of the primitive reptile groups. Others appear to be early ancestors of the dinosaurs and other reptilian forms; but by far the most interesting and abundant animals are a varied host of reptiles which belong to a group leading to our own relatives, the warm-blooded mammals.

Three quarters of a century ago fragments of bones from this region attracted the eye of a military engineer building roads across the interior of Cape Colony and were shipped to London to be examined by Sir Richard Owen. He, despite antipathy to the then newly expounded Darwinian ideas, could not help admitting that the greater portion of them appeared to be representatives of a group of reptiles intermediate in structure between the primitive reptiles and mammals. Later H. G. Seeley, of King's College, London, worked to some extent in this field. Our present interest in this fauna, however, is mainly due to the work of Robert Broom. A young Scotch doctor, much interested in the origin of mammals, he went to South Africa just before the opening of the present century and has spent his life in the study of these early ancestors of ours. He has described a vast array of interesting types, and has been able to demonstrate that we are actually dealing with a group which was structurally antecedent to the mammals. Watson, of University College, London, has, through careful morphological studies, added depth to the picture painted by Broom, while Haughton, of the South African Museum and the Geological Survey of the union, has contributed much to our knowledge of the fauna.

Walker Museum of the University of Chicago has long been interested in Permian vertebrates. More than a dozen trips to the Permian beds of Texas by Miller have yielded a large amount of material, which was described by the late Professor Williston and his students and which has added much to our knowledge of the early land verte-

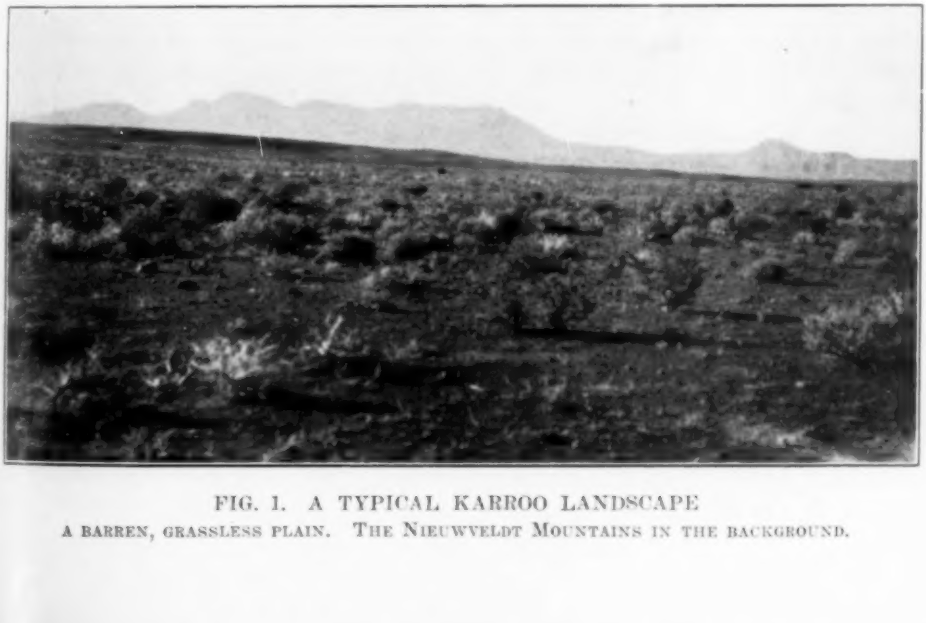


FIG. 1. A TYPICAL KARROO LANDSCAPE
A BARREN, GRASSLESS PLAIN. THE NIEUWVELDT MOUNTAINS IN THE BACKGROUND.

FIG. 2. LOOKING DOWN ON MILLER ALONGSIDE A PAREIASAUR SKELETON

brates. We have wished to expand the museum's work in this Permian field. But, as we have seen, this has been impossible in America. Our eyes for years have been turned toward South Africa as a region in which we desired to work.

This field, as I have said, was not an untrodden one. But despite the fine accomplishments of the few men who have worked on these Karroo vertebrates, the fauna is such a vast one that great lacunae still exist in our knowledge, and we felt that Miller's Permian field experience, if utilized in South African work, might result in the collection of material of considerable scientific value. And so it was with high hopes that Miller and I early last year found ourselves enabled, through the generosity of an anonymous Chicago citizen, finally to make the venture.

We landed last April at Cape Town, where we spent nearly two weeks obtaining supplies, a light truck and—just as important—advice. We found our scientific colleagues in South Africa most helpful. Dr. Haughton, of the South African Museum, gave us much good advice based on his personal knowledge of the Karroo, and later, when we got in touch with Dr. Broom, he too was able to furnish us with many valuable hints.

Out from Cape Town we traversed the narrow coastal plain and then began the crossing of the great ranges which parallel the coast. Over them the roads are, fortunately for us, well constructed, for they were made as military roads nearly a century ago. Railroads then were undreamed of, and good communications had to be furnished with the interior, where there was then intermittent fighting with the natives. Farther inland, the roads fade out. The main highway between South Africa's two

largest cities, Cape Town and Johannesburg, became merely a rut which it is hard to follow across the veldt. There are no bridges; when the road reaches a river, one plunges down the bank and makes his way as well as he can across the sandy river bed and up the opposite bank. Usually the rivers are dry. If it rains—well, one simply camps on the bank and waits a few days until the water runs off.

Four hundred miles across country, and we had reached our first destination, the edge of the Great Karroo. The term is a Hottentot one, meaning a desolate country, and we found no reason to disagree with this description. The region consists of a series of plains and plateaus, with an elevation ranging from two to six thousand feet, extending across much of the interior of Cape Province. To the south, high ranges cut off moisture-laden winds from the coast, and the prevailing winds are from the barren deserts to the northwest. The rainfall is consequently very small. In the poorest part of the Karroo, in which we first worked, it was, on the average, four inches a year, and in some sections there had been not a drop for five years. There is no grass, and agriculture is, of course out of the question. There are, however, small drought-resisting bushes on which sheep can somehow manage to make a living, and so the country has been settled by a hardy race of Boers. Since it takes on the average ten acres to support one small sheep, it may be imagined that their ranches (optimistically called farms) are necessarily of large size and the density of population very thin. About every hundred miles or less, however, there is a small "dorp" where supplies may be obtained.

Nothing more unlike their native Holland than this present home of these Dutch settlers can be imagined. (There is a story of a modern Hollander who was persuaded to buy a farm and emigrate to the Karroo. He stayed just six months, and then abandoned the "farm" and fled.) Their hold upon a livelihood is a precarious one. The only saving grace is the cheapness of native labor; ten shillings and a bag of corn-meal a month is the average wage. But despite their poverty, they are an extremely hospitable people, and we have warm recollections of the many kindnesses which they showed us.

Our first camp was made in the lowest of the many fossil-bearing zones into which the Karroo rocks are divided, in the district between Beaufort West and Prince Albert, Cape Province. We had been warned of the hardness of the rocks and the scarcity of fossils, and we soon found that conditions were as had been predicted. The first day Miller found a good prospect and next morning started optimistically to excavate his find. But when he returned to camp that evening he was quite despondent. He had broken the point off his favorite pick, had broken a cold chisel and had made no impression on the rock. The blue-gray mudstone in which the bones are imbedded is so hard that it is only where a skull or skeleton is weathering out on a flat surface and the rock has largely disintegrated that it is possible to do anything with it. In a number of cases we found good specimens only to leave them in position when we realized that we could not dislodge them without using high explosives which would merely reduce the bone as well as the rock to scrap.

There followed days of almost fruitless wandering over the arid veldt. The fossils were there, and it required merely time and patience before we should make a "strike." Finally, after a week of searching, Miller made our first good find, a practically complete pareiasaur, similar to the skeleton shown in the figure. These forms are rather large

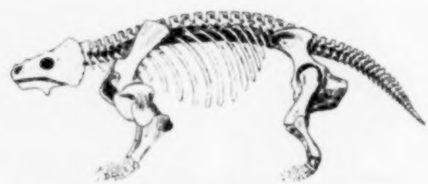


FIG. 3. THE SKELETON OF A PAREIASAUR
AS RESTORED BY BROOM.

and uncouth reptiles, a dozen feet or so in length, relicts from the primitive reptilian fauna and fairly closely related to some of our earlier American types. There are already two mounted skeletons of these curious creatures, one in Cape Town and one in London. We obtained two nearly complete specimens, as well as a large amount of more fragmentary material. One skeleton we hope to mount in natural position; the other, lacking part of the skull, will be exhibited as a slab mount, lying in the position in which it was found in the rock.

The life restoration figured is inaccurate in some respects, but gives an idea of the general appearance of one of these large and uncouth creatures. The best comparison that can be made is to imagine a horned toad from our western deserts about four yards in length. These reptiles were harmless herbivores, subject to attack from carnivorous enemies; and in correlation with this fact we find that there were series of bony plates down the back, the beginnings of a defensive armor.

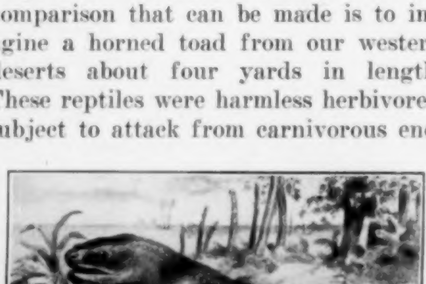


FIG. 4. A PAREIASAUR
AS RESTORED BY SMIT, IN HUTCHINSON'S "CREATURES OF OTHER DAYS." IT IS INCORRECT IN SOME PARTICULARS, BUT GIVES A FAIR IDEA OF THE GENERAL APPEARANCE.

It will be noted that this skeleton was discovered naturally articulated, "top side up," with the limbs extending down at its sides. The head and tail are the highest portions; the middle of the back is caved in (a feature incorrectly attributed to the animal in life by the restorer). Previous writers had commented on the fact (which we found to be true) that practically all pareiasaurs are found in this position, whereas almost all other animals in these beds are disarticulated. The suggestion has been made, very plausibly, that these pareiasaurs lived in the swamps and mud flats which then covered the region and that the skeletons are those of animals which were actually mired in the mud which hardened into the rock now forming the mudstone matrix.

FIG. 5. A HARMLESS HERBIVOROUS DINOCEPHALIAN
WITH A SWOLLEN FOREHEAD, FROM A SKULL IN THE BRITISH MUSEUM (AFTER WATSON).

Parenthetically, it may be noticed that here, as in later cases, it is the finds of other, earlier workers that I have figured rather than our own. The reasons are, of course, fairly obvious. The casual visitor to a museum seeing a mounted skull or skeleton of a fossil animal may

FIG. 6. A RESTORATION OF THE SKELETON

OF A LARGE CARNIVOROUS DINOCEPHALIAN, ABOUT FIFTEEN FEET IN LENGTH (AFTER BROOM).

imagine that once the specimen is found in the field and shipped home to the museum, little remains to be done. But in reality, work has barely begun. Freeing a single skull from the matrix of rock which surrounds it is often, under the best of conditions, a task requiring weeks or months of careful work. The Karroo rock has a deserved reputation for toughness, and the preparation of these specimens is far more difficult than usual. It will be years before a considerable portion of our material will have been prepared and placed on exhibition.

A second type of reptiles, of considerably greater scientific interest but tantalizing because of the usual fragmentary nature of the remains, is that represented by the Dinocephalia. The scientific name, "giant heads," refers to the fact that the skulls were enormous, for those days; one which we brought back measures nearly a yard in length. Over the eyes there was, in many cases, a huge swelling, giving a very high-brow appearance to the beast. This, however, was deceptive, for these reptiles were small brained, and this imposing forehead was composed of bone. Some of these giant heads were carnivores; others harmless herbivores. The carnivorous types are very close, it is believed, to the common stock from which the other South African mammal-like forms and their descendants, the mammals, have been derived; the herbivores were a sterile side branch. (It is interesting, by the way, to note that in the evolution of vertebrates it is almost universally the carnivores which have been successful in the long run, and given rise to higher forms, while herbivorous types have their day and then usually disappear without descendants. This is hardly in agreement with some of the pet notions of to-day; and if there is a moral

FIG. 7. A HOTTENTOT SHEEP-HERDER AND HIS SON

AT HOTTENTOT'S RIVER. THEY FOUND US A NUMBER OF GOOD SPECIMENS, INCLUDING THE SKELETON SHOWN IN FIG. 2. HE HAD NO IDEA OF THE LOCATION OF AMERICA BUT, AS MAY BE SEEN, HE IS WEARING A CAST-OFF ARMY BLOUSE.

to be drawn from it, I'm sure I don't know what it is.)

Our collections include skulls of both types, but unfortunately no complete skeleton, for, unlike the contemporary pareiasaurs, the remains of these forms are almost always found scattered. Only in two cases have associated skele-

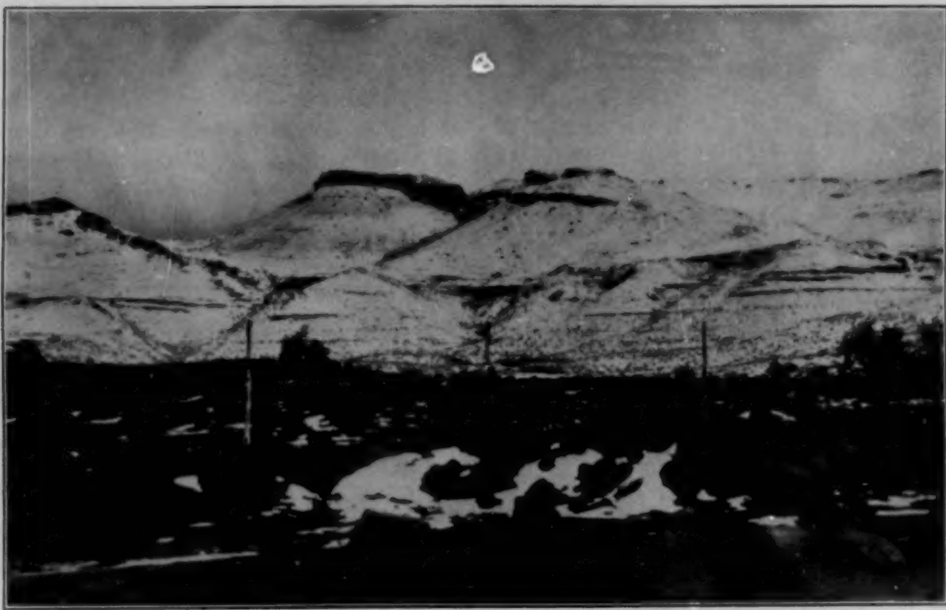


FIG. 8. WINTER COMES TO SOUTH AFRICA
SNOW IN THE NIEUWVELDTS, BEHIND OUR CAMP.

tons been discovered; and we were not fortunate enough to find a third exception to the rule. It is generally believed that these forms lived ordinarily in the higher land surrounding the swamps in which their remains were buried, and that the remains which we find are those of carcasses carried down by streams and broken up on the way.

After more than a month at our first camp we moved some fifty miles farther north to Hottentot's River. We were still in the same zone, but close up under the Nieuwveldt Mountains, which rose some three thousand feet above us to the north of camp. The place owes its name to the fact that this district was still retained by a Hottentot chief after most of the adjoining districts had been settled up. One McPherson, happening by after taking Scotch leave from his ship, married his daughter and inherited the farm. But Scotch thrift is becoming somewhat diluted with the passing of several generations, and we understood that the local villain holds a mortgage on the place, and by now may have foreclosed on McPherson's somewhat dusky descendants.

Collecting here was excellent, including a second pareiasaur (the one figured) and many other finds. We were especially fortunate in finding an unusually intelligent Hottentot herdsman. He had roamed these hills for years with the sheep, and knew every stone on the place. When we finally got him to understand what we wanted, he and his small son were able to lead us to many good prospects, saving us many days of search.

Our memories of this region, however, are somewhat embittered by the weather. It was July, in Africa. But July is, of course, midwinter in the southern hemisphere; we were in the south temperate zone, rather than the tropics (at about the latitude of El Paso) and in the mountains as well. Snow might have been predicted; and snow came. The natives told us that it was the worst storm in fifty years, but that was chill

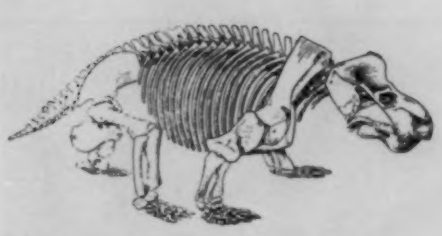


FIG. 10. RESTORED SKELETON OF A DICYNODONT (AFTER H. S. PEARSON).

comfort to us as we huddled about our fire, and we were only too glad when we decided that our bag from there was sufficient and that we should move on to new fields.

FIG. 9. A DICYNODONT SKULL (AFTER JAEKEL).

We now went farther north and east in the Karroo in among the mountains, in higher beds with a changed fauna. Here we found very few pareiasaur remains, and the "giant heads" had completely disappeared. Instead, the com-

FIG. 11. LIFE RESTORATION OF A TUSKLESS FEMALE DICYNODONT (AFTER OSBORN).

FIG. 12. A GULLY NEAR BETHULIE

MANY OF THE APPARENT BOULDERS IN THE BOTTOM ARE REALLY NODULES CONTAINING DICYNODONT SKULLS.

mon fossils were those of dicynodonts, "two-tuskers."

These forms, like the dinocephalians, were relatives of the mammal-like reptiles, with an advanced type of locomotion in which the limbs are partially brought around under the body rather than sprawled out at the sides in the fashion of primitive reptiles. But their skulls were grotesque. Amongst other peculiarities these strange creatures had lost practically all their teeth. In many cases there was a single pair of tusks in the upper jaw, placed where those of a carnivorous mammal should be, but the rest of the mouth was toothless, and

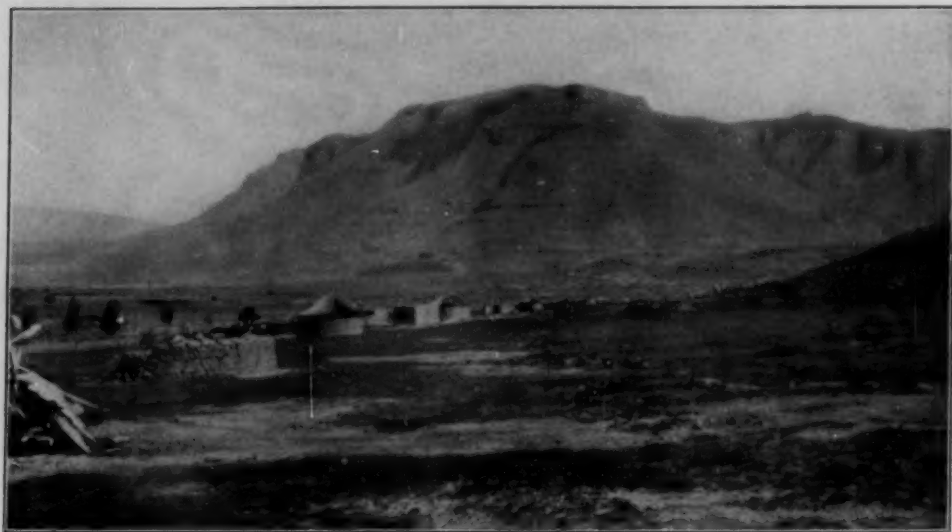


FIG. 13. THE CYNOGNATHUS BEDS NEAR LADY FRERE
THIS MORE FERTILE COUNTRY IS THICKLY POPULATED BY "KAFFIRS," SOME OF WHOSE KRAALS MAY BE SEEN IN THE FOREGROUND.

seems to have been covered in life with a horny, turtle-like beak. Some even lack the pair of upper tusks. These were placed by the older writers in a separate group. Later, however, it was suspected—and it appears to be true—that this difference was often merely a sexual one, the male being tusked and the female tuskless.

At our first stopping place in this zone, Wagenaar's Kraal, the people were of the pleasantest, the fossils of the poorest, and so regretfully we moved on to Murraysburg, a little Dutch "dorp" at the foot of the Sneewbergs. Here we were, comparatively speaking, in the heart of civilization (population 750). There was even a hotel. But we found, after a brief experience, that our camp cots and our own cook's very limited culinary offerings were, after all, not bad by comparison.

Fossils here were plentiful. Dicynodonts large, dicynodonts small, dicynodont skulls, dicynodont skeletons and a sprinkling of rarer things, such as skulls of more typically mammal-like forms and one tiny skull which, although only two inches long, is that of a type believed to be the ancestor of the great dinosaurs of later days. On one hillside finds were so plentiful and so easily obtained that we had to hire a Negro boy to caddie for us to carry loads of fossils from the shale exposures on the hill to the car.

Still farther northeast we pushed to look into the fossil offerings of still higher beds, across the Orange River and into the southern edge of the Orange Free State, near Bethulie. Here we were told that we would encounter remains of *Lystrosaurus*, an aquatic two-tusker, a sort of reptilian seal. And we found them, as usual of course, in the most unlikely spot. Coming along the road we spied a small and rather poor-looking exposure on a small hill, and agreed to stop ten minutes or so, before we went on—"just to be sure there's nothing there." There was nothing on the bare spot. But just below was a small gully cutting through the beds of shale. Into this Miller,

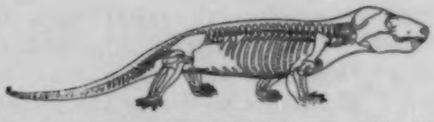


FIG. 14. THE SKELETON OF AN ADVANCED MAMMAL-LIKE REPTILE
CYNOGNATHUS, AS RESTORED BY GREGORY AND CAMP.

with a nose for bones, promptly plunged, to find that what appeared to be a large number of boulders in the bottom of the ditch were really nodules, each containing all or a part of the skull of the creature for which we were looking.

The very highest parts of the Karroo Series center around Basutoland, an almost independent native state in the mountains in the heart of the South African Union. These beds contain a fauna consisting principally of early dinosaurs and their relatives, in which we were not particularly interested. But below them—and higher in the series than we had yet worked—were the Cynognathus beds, which we must investigate in the hope of enriching our collection—all too scanty—of real mammal-like reptiles, the theriodonts.

The beds in which the best-known members of this group occur were well delimited on our maps. But it is known that most of their extent appears to be absolutely barren of fossils, and the known localities had been so thoroughly worked over for specimens of these rare forms that it was hopeless to repeat the search. On this account we were forced to make a stab in the dark and explore comparatively new territory. We first tried the country about Rouxville in the southern part of the Free State. But ten days of search in fine-looking exposures gave us only a few almost worthless scraps. So we decided to make a final attempt, and crossed to the south over the Stormbergs back towards the coast to Lady Frere, where some of Seeley's early finds had been located, and where we had somewhat better luck.

These cynodonts are the highest development of the reptilian ancestors of the mammals, and show many striking resemblances to our warm-blooded relatives. The whole skeleton, especially the limbs, is much more mammal-like than in any other group of reptiles, while the skull is curiously suggestive in many ways of that of such a generalized higher type as the opossum. The teeth, for example, are differentiated into incisors, canines and molars; as in the mammals, the head joins the neck by two condyles, in contrast with the single condyle of ordinary reptiles; the nostrils, instead of opening directly into the roof of the mouth, as in normal reptiles, are sometimes separated from the oral cavity by a hard palate. The Karroo beds are practically the only place where these forms are to be found. In later times, the development of the dinosaurs seems to have caused the extinction of these ambitious carnivores—all, that is, except that one small group which was destined to become the ancestors of the mammals.

We were amazed at the change in the country in the Lady Frere region. We were now back on the coastal side of the range where the rainfall is greater. Here we saw for the first time in six months grass-covered fields on which even a cow could make an orthodox living. Further, while our previous locations had been in country occupied by white farmers, we were now in a

FIG. 15. A SKETCH RESTORATION OF CYNOGNATHUS (AFTER OSBORN).

native district, with Kaffir kraals all about us. Lady Frere is a tiny hamlet in which reside the officials and traders who deal with a surrounding black population of fifty thousand. It proved a delightful stopping place.

It was October by the time we had finished our work here, and the time was approaching when we were scheduled to return to America. And so we shipped our last specimens and trekked back to Cape Town, headed for home.

Our collections are safely back in Walker Museum where the sound of Miller's hammer and chisel may be heard daily as he works on the intractable matrix. The preparation of our finds will be a long and tedious process. But in time this will be accomplished and we can only hope that their study will contribute in some degree, at least, to the scientific knowledge of these ancient reptiles that lived so long ago in Southern Africa.

AN ARCHEOLOGICAL RESEARCH AND ITS RAMIFICATIONS

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For the past fifteen years Carnegie Institution of Washington has been engaged in archeological studies in Middle America, the project centering in Yucatan and concerning itself primarily with the career of the Maya Indians, builders of the highest aboriginal civilization of the new world. The investigations have added to our understanding of the intellectual achievements of the Maya in mathematics and astronomy, have served to point out strategic areas for intensive excavation and, most important of all, have aided in establishing, upon the sure foundation of a dated chronology, the sequence of the principal categories of Maya remains. But proper utilization of these data, in other words their interpretation in terms of history, can be made only in the light of accurate information as to the biological nature of the populations concerned and as to the environment in which they lived. These factors form respectively the raw material of and the setting for the course of historical events, and without understanding of them it is impossible to reach valid historical conclusions. It has accordingly been necessary to call for aid by workers in several non-archeological fields. Furthermore, the principle that in any investigation one should proceed from the known to the unknown, which in archeology means that one should work from the known present back to the unknown past, has induced analysis of modern conditions and consideration of post-conquest history. All this has brought about a concentration in the peninsula of the following researches: Archeological work at Chichen Itza, under direction of Dr. Morley, now in its seventh year; excavation at Uaxactun, Department of the Peten, Guatemala, under direction of Mr. Ricketson, fifth year; hieroglyphic research by Dr. Morley, twenty-fifth year; ceramic survey of the Maya area by Carnegie Institution, inaugurated in 1930 by Mr. Roberts; medical survey of Yucatan by Harvard University and Carnegie Institution, now in its second year; records of the Chichen Itza clinic, third year; biological reconnaissance by University of Michigan, Dr. Gaige; ethnological reconnaissance for Carnegie Institution by Dr. Redfield, University of Chicago; studies of Maya linguistics by University of Chicago, Dr. Andrade, all in their first year. Proposed activities are historical work on the conquest and the colonial period; retranslation and collation of native chronicles; investigation in physical anthropology by the department of genetics; geological, meteorological and agronomic reconnaissances; air survey of the Maya area.

A conference attended by persons directly connected with the above activities and by certain others who were willing to give the institution the benefit of their counsel was held during January at the institution's field headquarters at the ruins of Chichen Itza. The general project was reviewed, and the bearings of its several phases were discussed. Finally, it was possible to formulate a number of problems, specific and general, and to lay tentative plans for

attack upon them. We venture to summarize some of the results as an unusually clear example of the close interrelation which exists between historical research and the natural sciences. They also seem to indicate that coordinated attack upon the highly complex subject of man's career offers unusually effective means for bringing together, both practically and intellectually, workers in different branches of natural science who, in the ordinary course of their studies, would have few opportunities for contact with each other.

Upon archeological questions and archeological method we touch most briefly. But as Maya history is the backbone, so to speak, of the investigation, it is necessary to state that the culture of this interesting people arose, from sources not as yet clearly identified, during the first millennium before Christ. The peak of Maya progress was reached in cities lying at and near the root of the Yucatan Peninsula, a region which was occupied for at least a thousand years and which was suddenly and very mysteriously abandoned about the beginning of the seventh century A. D. Although the old home land was given up and the country reverted to jungle, the Maya persisted on the Guatemala Plateau and in the plains of northern Yucatan. In the latter area they achieved a brilliant renaissance, engaged in severe intercity struggles, suffered invasion from Mexico and were apparently upon the verge of complete political decadence when the Spanish conquest put an end to native civilization. The Maya, however, still form the bulk of the population of Yucatan, and the thread of Maya history therefore runs unbroken to the present day.

Judgment as to Maya origins involves many cultural considerations, but two of the key problems are essentially biological. The first of these concerns the somatological relation between the Maya and other aborigines of Middle America. It must be answered by the physical anthropologist. The second question relates to maize, or Indian corn, for upon that cereal the Maya, and indeed all other new world civilizations, were founded, as those of the Mediterranean were based upon wheat, and those of the East upon rice. Hence the inquiry shifts to the realm of the botanist and the student of plant genetics. The archeologist must depend upon the results of these sciences for information as to the identity and range of the wild ancestors of maize, which he requires for locating the place of origin of American agriculture. The archeologist may perhaps, however, in part repay the debt, as he has already begun to do in southwestern United States, by furnishing ancient specimens of maize from long series of horizons, thus providing the geneticist with datable material for calculating the rate of development of recent varieties.

A very important element of Maya culture was its remarkably accurate calendrical system. The desire to record passage of time seems, indeed, to have led to the invention of Maya hieroglyphic writing. At all events the carved dates of the inscriptions (which are decipherable) give us a precise relative chronology for the principal ruined cities. The details of epigraphic study can not be considered at this time, but it may be remarked that the most pressing question at present is the exact correlation between the Maya and the Christian calendars, a correlation which is now approximate, but which, according to the best authorities, can ultimately be made exact by astronomical and mathematical studies. In the case of astronomy it is to be feared that archeology can make no return for benefits received, but to the history of mathematics and the philosophy of numbers there are offered most interesting materials, for of

all mankind the Maya first arrived at the concept of zero and first practiced positional arithmetic.

Field and laboratory methods in archeology need not concern us here. One may say, however, that success depends upon intensive technological research leading to recognition of chronologically significant criteria, ceramic, lapidary and architectural; upon determination by stratigraphic means of the relative age of materials embodying those criteria, and upon comparative studies to establish the range and to evaluate the importance of different categories of specimens.

The results of epigraphy and archeology can, as has already been said, be understood only in the light of knowledge of environmental conditions. For this we must turn to the biologist, the geologist, the geographer and the meteorologist.

Biological research in Yucatan has hitherto been sporadic and of necessity superficial. It is hoped, however, that the reconnaissance made during the past winter by Dr. Gaige may lead to the undertaking by University of Michigan of an extended ecological investigation. The mere stock-taking of flora and fauna involved in such a project would be of much value to taxonomy and biogeography. It is also absolutely essential to the student of man, for it concerns such basic matters as the nature and extent of resources in game and in food plants, as well as animal and vegetable products useful in the arts, to say nothing of the listing of deleterious factors, such as disease-carrying organisms, noxious species, etc. There are also less obvious questions upon which the biologist can throw light. The *cenotes*, for example. These are well-like breaks in the limestone crust of the peninsula caused by collapse of the roofs of subterranean water-eroded chambers; Yucatan has no surface drainage, therefore in ancient times the only permanent water supply must have come from the *cenotes*. Leon Cole has shown that comparison of the faunas of these tanks can probably be used to determine the extent of underground circulation, a phenomenon which has important bearing upon pollution of drinking water and so upon health conditions. Also from the excavator's standpoint the size of the openings between the *cenotes* is of moment in judging the practicability of pumping them out to recover the wealth of votive objects which were cast into some at least of them.

An ecological survey will have all the usual value of such work. In addition, Dr. Gaige has pointed out certain special studies which can profitably be pursued in Yucatan. These concern adaptations in species, trapped, so to speak, in isolated *cenotes* where they have existed under naturally controlled conditions for varying periods of time presumably approximately measurable by geological methods. Another type of adaptation is illustrated by terrestrial forms, which must have emigrated from the humid tropics as Yucatan emerged from the sea, and which have fitted themselves for life in the arid, subtropical environment of the peninsula. Here again a time-scale can probably be supplied by the geologist, and there may consequently be gathered data as to rate of modification, persistence of traits and other phenomena of fundamental genetic and evolutionary importance.

Dr. Gaige has outlined still another line of investigation, and in this the archeologist can be of assistance. The Maya undoubtedly cultivated very extensive acreages of corn, for the vast extent of their building operations argues not only a great population but a large surplus of food. Forest destruction, either by *milpa* agriculture or by the making of permanent fields, must, therefore, have been enormous. Careful

plotting of the location and size of ruins can in all probability be counted upon to indicate the areas of such denudation, while hieroglyphic, ceramic and architectural studies will determine its age. The biologist may consequently be provided with definite regional and chronological information for research on the succession of floras and faunas and on the rate and order of their return to artificially cleared terrain.

At this point the agronomist enters the picture. He must pass upon soil capacity and former agricultural methods. His decisions are vitally necessary to the archeologist, for the social and economic structures of the Maya were molded by their farming practices. And the judgments of the agronomist working in conjunction with the ethnobotanist will have to be taken into account in all researches which touch in one way or another upon questions of nutrition. For example, Dr. McCollum believes that maize is only adequate as a staple diet under one of two conditions: when there is a superabundance of sunshine, or when maize is supplemented either by green-stuffs or by the organs of animals subsisting on green-stuffs. Now maize civilization originated in arid, sunny plateaus; it eventually spread to less sunny lowlands (as in the ancient Maya country at the south of Yucatan); in many such regions, presumably because of higher returns from agriculture, civilization flowered astonishingly; but increasing population probably eliminated game, the Indians were never given to raising greens and when the Spanish arrived the high maize cultures were either dead or dying in every part of the new world save in regions of excess sunlight. We would not push speculation based on these facts too far, but such thinking adumbrates researches of undoubted significance for the historian, the biometrician, the nutritionist and the student of health conditions.

This brings us to the medical work. Dr. Shattuck and his group from the Harvard department of tropical medicine have now spent two seasons in the field. They have made clinical examinations, have gathered statistics and have conducted specific studies of certain prevalent diseases. They have found the country, for the tropics, unusually healthy, but intestinal and respiratory troubles prove to be extremely common. Yucatan, according to Dr. Shattuck, is admirable territory for studying the very important and little understood tropical anemias, because malaria, which as a rule is so abundant and whose symptoms overlie and becloud the more subtle manifestations of anemia in other regions, is here relatively rare and its effects can, in a general sense, be factored out. As a first step in this investigation Dr. Shattuck has undertaken the determination of normal standards, to be followed by nutritional research, checks of the bacillary and amebic dysenteries and of other pertinent factors. Extension of the work will lead to consideration of the whole question of life in the tropics, involving the human geography of all tropical countries and the ability of various peoples to exist under conditions of heat and humidity. There are naturally involved matters of race, of climate, of food supply, of native and introduced diseases and of the physical properties of the sun and the atmosphere.

General information as to health and accurate knowledge of the nature and the history of specific diseases are, of course, needed by the historian as well as by the student of present-day conditions. The medical man, on the other hand, can not carry his labors very far without being forced to call upon the archeologist for estimate of the location and thickness of early populations; on the paleopathologist for evidence of ancient disease; on the ethnologist for

data as to manners and customs which influence the spread of sickness, and on the biologist, geologist and meteorologist for all sorts of environmental data.

Certain medical investigations are of interest to the historian, to the immunologist and to the student of the spread of diseases. Dr. Shattuck found in 1929, for example, that the Maya Indians were remarkably free from syphilis, whereas the Spanish and mixed bloods were normally infected. Does this indicate immunity derived from prehistoric prevalence of the disease, or have social conditions acted to prevent its diffusion? The matter is being looked into this year. The results may supply evidence upon the vexed question of the new or old world origin of syphilis. Yellow fever, again, has been held responsible by Spinden for the break-up of the Maya Old Empire; but yellow fever is generally thought to have been introduced after the conquest. The malarias, too, are to-day the single worst malady of the American tropics. Were they prevalent in ancient times? The documentary historian, by scrutinizing the records of early expeditions into now infested regions, may well be able to supply pertinent facts.

Work in medicine is intimately connected with that in physical anthropology, not merely because both sciences are concerned with biological aspects of man, but because both deal with basic problems of race, medicine approaching the subject by use of physiological criteria, somatologists mostly by the methods of anthropometry. The recognition of pure, or relatively pure, races is difficult enough, but when one encounters, as in Yucatan, all degrees of hybridization, the task of establishing degrees of admixture becomes so complex as to require use of every possible category of evidence. Anthropometry, however, is undoubtedly the readiest means for arriving at preliminary classification. An excellent start has been made by Dr. Williams under the joint auspices of Harvard and Carnegie Institution. Eighteen hundred individuals have been measured, many blood-samples taken and many observations made on basal metabolism. The results, which are shortly to be published, should serve as foundation for the study, already mentioned, of the place of the Maya in the aboriginal population of America and for identification of the particular Caucasian strains entering Yucatan since 1545, together with the various degrees of Indian-White crossing. Clinical medicine, genetics, sociology, psychology and all other disciplines dealing with race-linked phenomena will directly or indirectly be benefited by continuation of this study.

Geological investigation has not as yet been undertaken, so that it is not possible to make statements concerning the specific questions which will prove of interest to workers in that science. But the findings of geology are essential for many other branches of the survey. The archeologist must have knowledge of the mineral resources of the peninsula in order to determine whether or not objects found in the excavations are of local origin, and to gauge both the amount and the provenience of mineral imports. The ceramicist must learn the extent, location and relative accessibility of sedimentary deposits containing clays. Questions of soil and of water supply are of interest to every one, and are particularly important for the medical group and the agronomists. The age of the peninsula and the rate of its emergence are, as has already been brought out, of great significance for the biologist.

Climate is still another factor of environment which has to be taken into account. The applications of meteorology are naturally legion. Statistics, published and documentary, are now being gathered, and local records are

kept. It is hoped to add further observational equipment when a competent meteorologist can have looked over the field, and shall have consulted as to their special needs with the workers in other sciences.

Geographic data are a by-product of practically every phase of the survey. Together with other statistics they are being collected, by means of the duplicating note-books used by all field men, at division headquarters in Washington. But just as anthropology occupies a central, synthesizing position among the sciences of man, so geography serves to bind together those of environment, and there is, accordingly, need for association with the project of men trained in the latest methods of that science. Particularly will they be helpful in the planning and prosecution of the air survey of Yucatan which it is hoped may shortly be made. By means of the plane the topography of the whole Maya country could be plotted and the distribution of its forest types recorded. Parties on special missions, biological, geological, archeological, could quickly be transported and set down on lakes or rivers otherwise accessible only to large, expensive and time-consuming expeditions.

And so one might indefinitely go on pointing out lines of research and their bearings upon each other. The difficulty lies not in what to do, but in selecting what is most important to do, in deciding how best to do it and in finding the men and the means with which to get it done. No one institution can possibly handle a project so large and so varied. If ultimate success is to be attained it must come through realization by many agencies that the field is a significant one, through confidence that it is being developed in the proper way and through belief that investment of effort in cooperative research will bring valuable scientific dividends.

A few points deserve emphasis. Administrative organization, it is thought, should be flexible. Program must not be rigid. Individual effort should be directed by individual scientific interest. Studies should be coordinated, but they should be carried on independently, should be highly specialized and should pursue definite goals within the limits of the sciences they represent. Any given unit of research so conceived and so prosecuted may be expected to contribute its quota of facts to the corpus of information at the disposal of its mother science, and it should also have influence in development of the methodologic practice and the underlying philosophy of that science. This, of course, is true of all proper investigations, no matter what their immediate aims may be and regardless of where they are carried out. But if, as in Yucatan, a number of such researches be undertaken simultaneously, if they utilize the same general body of materials, employ the best practical and intellectual weapons and if close touch be kept among the workers, it seems obvious that progress must be greatly accelerated, because data will be cumulative and results will be comparable. Mutual concern with inevitably interlocking problems will induce the most natural and therefore the most effective form of cooperation. And without cooperation we can never cope with phenomena too large for comprehension by any one science, but which lie at the root of many sciences. The Walrus, after all, was right. Seven maids and seven mops will eventually make their impression. He only erred in expecting them to finish their job in half a year.

AN EXPERIMENT IN TEACHING

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PERHAPS no teachers in this country give less consideration to or take less stock in pedagogical theory and practice than do college professors. Few college or university teachers, in the biological sciences at least, have ever had special training in the theory and practice of teaching. Whether this is necessary or even desirable I am not saying. It is my opinion, however, that much of the criticism now leveled at the college student because of his disinterest in his scholastic work ought rather to be directed against the professors who teach him. My own experience as a student and as a university professor, now of some thirty years, satisfies me that the average professor teaching college students has little "method in his madness." I myself do not pose as an expert in pedagogy, nor would I claim complete immunity from the above indictments. I only marvel that we who worship so devoutly at the shrine of scientific method should practice it so little in our teaching.

The procedure in presenting laboratory courses in biological subjects to-day is, with rare exceptions, that introduced half a century ago: one or more laboratory exercises per week in which all the students study the same subject and the same material under the solicitous guidance of immature instructors; a lecture or two by the professor, a mass recitation conducted by one of these same instructors or, rarely, by the professor himself, and a voluminous weekly report which is usually only a slovenly and inaccurate misalliance of poorly digested laboratory pickings. In addition to this there is frequently imposed upon the student the handing in for correction (sic) by the overburdened instructors of a written copy of the student's notes taken during the professor's lecture. This is rarely complimentary to the professor, of no interest to the instructors and of questionable value to the student. This, in brief, is the system in vogue, at least in most of the institutions of higher learning in this country.

During the first twenty years of my career as a teacher in Cornell University, I followed this general system with a degree of faith and devotion which I can now only contemplate with a feeling of astonishment and chagrin. During the second decade of this period, I began to be aware of a feeling that something was wrong with my teaching methods. In spite of the fact that, so far as I could learn, the courses which I gave were looked upon with a reasonable degree of pleasure and approval by my students, I still felt that there must be a better way. I sought in vain among my colleagues and the literature on the subject for a procedure which appealed to me to offer a direct solution for my difficulty. Ideas gained from articles I read on the Dalton system and other experiments then being initiated in secondary schools set me thinking. Out of this thinking grew a resolution to attack my teaching problem in the same way that I was accustomed to attack research problems in my field. I decided to experiment. Having won the approval of my instructing staff of this point of view, I spent an entire summer in working out a new and untried procedure. We gave it a trial for the first time that fall semester. This experiment has now been going on for some ten years, and there is every indication that it will continue an experiment to the end of my teaching

days, for each year problems in the details of the method present themselves and each year we attempt by the experimental method to solve them and to perfect our procedure.

I shall now attempt to outline the procedure as it operates to-day and set forth something of the philosophy underlying it and some of the results which I think may fairly be claimed for it.

The course which I am about to describe, expressed in terms of our curriculum, is a three-hour elementary course in plant pathology, of one lecture a week and of nominally two laboratory periods of two and a half hours each. The course in the fall term is limited to fifty undergraduate students. If more than fifty register for it, that fifty having the highest average standing in all their preceding work in the institution are admitted. The remainder are rejected. In the spring term the number is limited to twenty-five on the same basis. Students who because of their low standing have been rejected in the autumn usually get the course in the spring term, when applications are relatively few. The course is open to sophomores, juniors and seniors throughout the university. The only prerequisite is the regular six hours' course in beginning botany. The teaching staff in this course normally consists of myself, an assistant professor, an instructor and a laboratory assistant with an assistant in charge of what is known as the "materials room."

When the students arrive for the first exercise, they are given some mimeographed information sheets in which the manner of conducting the course is briefly set forth. The salient points in the procedure are verbally emphasized at this meeting with the class. The students are then referred to a list of exercises in the information sheets. Here are found fifty-eight exercises only three of which are prescribed for all students. The remaining ones, each dealing with a specific disease of certain plants, are arranged in twelve groups. From each of these groups the student is permitted to select one for study, thus making a total minimum of fifteen exercises to be done during the term. Each student is allowed to select an additional five or more diseases which he may study if time and inclination permit. The average number of exercises completed per student per term is around seventeen.

Having made his selection of exercises with the advice and approval of one of the professors, the student is now ready to begin work at once. He goes to the materials room adjoining the laboratory, where there is checked out to him by the assistant in charge the necessary mimeographed text on the subject, preserved specimens, cultures, slides, photographs and other materials illustrating the disease to be studied. Each student is allowed to have out, at any time, materials for the study of three different diseases, and may with permission of the professor in charge check out more than this under certain conditions. He purchases at the bookstore our printed "Laboratory Outlines" in which he finds directions for study of the laboratory materials, and supplies himself with some plain sheets of drawing paper and drawing pencils. He goes to the department stockroom, where he checks out a complete equipment of laboratory tools, and gets his assignment to a locker. The microscope which he is to use, bearing the same number as his locker, is kept in a case in the laboratory. The specimens and materials which he has checked out from the materials room are handed to him in a shallow tray which slips into his locker. The locker will hold from three to five such trays of material. He is assigned to a table in the laboratory which is his on two specified laboratory periods during the week, though he is free to use it or any unoccupied table in the laboratory at any time throughout

the week, day or night. A laboratory which opens its doors to students at nine and closes them against him at four is an abomination.

The students are now told that they are at full liberty to come and go from their laboratory work as they choose. They are informed that during the so-called laboratory periods from 1:40 to 4 o'clock on three afternoons of the week and from 10:30 to 1 on Saturday morning there will be an instructor in attendance to answer questions and give them such assistance as they may request. The instructor is not permitted to offer advice or assistance to any student unless requested to do so. Outside of these periods students may not ask the instructor for assistance. They are quite free, however, to call on the professors for help at any time.

When the student has completed to his own satisfaction the work on any given exercise he applies for a conference test on the subject. This test may be taken only during the specified laboratory periods. As soon as he has thus prepared himself for the conference, he writes his name and the name of the exercise on which he wishes to have the conference on a small slip of paper and drops it into a slotted box at the materials room. This may be done at any time during the week. He may also designate on the slip the particular laboratory period during which he desires the conference. The slips are taken from the box at the beginning of each laboratory period by the assistant in the materials room, who then displays them chronologically in a special holder so placed that the student can see his position with respect to the others, in the order of call.

Promptly at the beginning of the period, the assistant professor and I appear at the materials room. One of us takes the slip of the first student on the list, the other the next, and each is called to a personal conference in another room. We have tried holding the conference with two or more students ready at the same time on the same exercise, but it does not work. Individual conferences are now the rule. We have in the conference room the necessary blackboard and other facilities for the work.

When the student comes to his first conference, the procedure involved is explained to him. He is told that we assume that he has accumulated a sufficient body of facts and information to enable him to solve a series of problems which will be set for him on the subject. It is pointed out to him that if he does not have the facts necessary for the solution of the problem he of course can not solve it, but it is also emphasized that he may have all the facts and yet be unable to do the mental operations necessary in manipulating these facts for the solution of the problem. In short, it is made clear to him that thinking with facts to solve problems constitutes the test of his accomplishment in his work.

The professor then proceeds to set problem after problem, some of them simple and some of them complex, and the student, standing on his feet without other students to embarrass him or to give him moral or material support, must by word of mouth indicate to the professor the mental operations through which he proceeds in his attempt to solve the problem and arrive at a logical answer or conclusion. When in his hesitation or stumbling it becomes evident to the professor that the student is having difficulties, an attempt is made by asking specific questions to discover whether it is the lack of knowledge of certain facts or the inability to use the facts he already has that is giving him pause in his mental attack on the problem. If the professor discovers that the student is wanting in knowledge of one or more essential facts for the solution of the

problem, one of two courses may be adopted: the professor may *give* the student the facts and then ask him to proceed with the solution of the problem, or he may decide that the paucity of factual knowledge is so great that "gifts" are not warranted, and he dismisses the problem with an explanation to the student of the unhappy situation in the case. He then proceeds to another problem in the hope that here the student will have the essential factual data. It is not the particular facts that the student knows or does not know that appear to me essential, but it is important that he shall have a sufficient body of facts so distributed over the subject as to enable him to operate with them in solving a reasonable number of pertinent problems.

The conference may continue for a half hour or an hour and a half, depending on the number of students who are waiting for conferences, the evident need of the student for mental exercise or for any one of a number of other reasons which will occur to any good teacher. A few well-selected problems will be sufficient to give any competent instructor, within the course of fifteen to twenty minutes, a just estimate of the student's accomplishment in the subject in hand. Thus the least important purpose perhaps of the conference, though a necessary one under our antiquated system of educational administration, namely, that of determining what grade the student should have for the conference, is quickly accomplished. The remainder of the time available for the conference can then be used for the more important purpose of teaching the student how to think with facts which he has accumulated and giving him exercise in this all-important feature of the process of education.

When the professor has completed his questioning the student is urged to present anything which he may have in mind in the way of questions for clarification. Such student questions are answered, where possible, by the Socratic method, thus compelling him if possible to think out the answer to his question himself. Such time as may still be available is often profitably devoted to giving the student new or interesting information along certain lines which would not otherwise have come to his attention in the study of materials or references to literature, and so his knowledge and interest in the subject may be broadened and deepened.

At the conclusion of the conference the student is asked whether he is prepared to accept a grade on his performance for that exercise. He may elect to accept a grade, after which the professor dismisses him and enters his grade in the records in the materials room where it is available to the student at any time after the conference. Or the student may elect not to take a grade at that time but to go back and study the disease further and apply for a conference at some later time, the only provision being that he must take the reconference with the same professor. A note to this effect is made by the professor on the student's card which is returned to the materials room assistant who files it to await a time when the student shall again sign up for a conference on this subject. The student may elect a reconference on any given exercise as often as he chooses with the distinct understanding that his grade will be based on the final performance, when he decides to take the credit. His performance in previous conferences has no bearing whatever on the grade which he finally accepts. Having once declared his readiness to accept his grade, however, he can not thereafter claim a reconference on that particular subject.

A student finding his name well down the list for conferences on any given afternoon need not waste his time wait-

ing for his turn. He proceeds with the study of the next disease of which he has material checked out, until he is finally called. If the number up for conferences on a given afternoon is greater than can be handled during that period, those near the end of the list will have to wait for a conference until a later period, but their names will appear at the head of the list at the next period on which they expect to be in the laboratory.

The attendance at the one lecture in the week is likewise not compulsory. The student may come or remain away just as he chooses without any detriment to his standing in the course, so far as the mere matter of attendance or absence is concerned. Not only is he told that he may remain away from the lecture if he chooses, but provision is made so that he will not be seriously handicapped by such absence. A complete, mimeographed text of the lectures is given out to each student at the beginning of the course. In this are set forth the organization of the subject, the important generalizations with ample illustrative cases and with a well-selected bibliography of references bearing on each subject so that the student will be able quite independently of the lecturer to get all the essential facts, generalizations and conclusions.

The lecture which comes once a week is not given, as is usual, for the purpose of conveying facts, theories, opinions and conclusions of importance to the student. Those he will find in the lecture text. The students are told this at the first lecture with the remark that if they wish to know the purpose of the lectures they may find out by coming to them. The real object of lecturing, as I see it, should be to stimulate and inspire the student to a real interest in the subject; to give "set" as the educational psychologists say and to vivify and adorn important subjects and generalizations. The student should go forth from a lecture with a new eagerness to examine his laboratory materials and with a renewed desire to explore further into the writings of those who have contributed to our knowledge of the subject. A professor who is not an interesting and stimulating speaker should never lecture either to college students or to anybody else. I never criticize a student for sleeping in one of my lectures. If I can not keep him awake he is entitled to all the sleep he can get. It is made clear to the students that no lecture attendance is kept, that they are quite free to come to none or only part of the lectures if they choose, or even to only a part of any one lecture. Coming late to a lecture or going out before it is completed is not a crime. Such incidents are of concern only to the students themselves. A professor who is disturbed by a student coming in late or leaving before he has finished speaking is too nervous to be holding down a teaching position in a university or college.

Yes, there are examinations in this course. One must defer to tradition and custom to some extent. Three unannounced prelims are offered during the term. The student may elect to take these or not as he chooses, but if he takes one he commits himself to the other two. There is a final examination, required of all students through the beneficent action of our faculty. I sympathize with my students, but hold them to it. The final term grade is made up as follows: 50 per cent. of it is based upon the average grade obtained in the conferences; if the student has taken the prelims the average of these counts 22 per cent. of the term grade, and the final examination 25 per cent.; if the student elects not to take the prelims, then the final examination counts 50 per cent. of the term grade.

This, I think, sets forth briefly the present procedure in this experimental course.

I shall now pass to some of the philosophy which I think underlies this procedure. I would point out, in the first place, that every possible responsibility for the work in the course is placed upon the student and is not assumed by the professor as is usually the case. The student is made to understand clearly that his attendance at lectures and laboratory periods is wholly a matter for his own decision. The professor requires nothing with respect to attendance, nor does he concern himself with it. It is assumed that every student taking the course is quite competent to adjust his own coming and going to his best advantage. Failure to do so brings with it certain consequences for which he alone accepts responsibility in registering for the course.

Some of you may wonder whether this procedure operates as effectively with students who are required to take the course as with those who elect it. I can assure you that it does. Approximately half of the students taking the work in the fall term are required to take it by the department of forestry. The department of plant pathology itself requires no student to take any of its courses, nor does the faculty of the college make any such requirement. Our experience has been that while the forestry students often approach it with the well-known attitude of the forced laborer, they quickly succumb to the novelty of personal responsibility involved, and we are soon unable to detect any marked difference between their attitude and that of those who have elected to take the course.

The question may also be raised in your minds as to whether under this system the student devotes the necessary time to the work in the course. You need have no doubts on this point. Inquiries carefully checked have proved that the average student spends nearly twice as much time per week under this system as he did under the old one. This results, perhaps, from the unaccustomed feeling of responsibility which is suddenly thrust upon him. He is not held either directly or by implication to set hours for work nor does any one suggest just how much time he should devote to the work. He has contracted to do a minimum number of exercises; some of them rather short and easy, others long and complicated. He soon learns from his conferences that it is not time spent with the subject but knowledge acquired and usable that is requisite to a creditable performance in the conference or examination. He is compelled to decide for himself how much time he will or can afford to spend on this particular course in relation to the other courses which he takes. The natural result is that he puts in the time required in the other courses, or as little of it as he can get away with, and devotes the rest of his study hours to the course in plant pathology. Students often remark to me that the course takes a lot of time and that if all the courses in the university were taught in this manner one couldn't carry many subjects at any one time. Quite right; but no student ought to expect to carry more than three or four subjects satisfactorily. Under the old system we dragged the students through twenty exercises of the same kind as those of which he now contracts to do but fifteen for the same credit. It is not that the course takes more time; he gives it more. I remind the student that his natural ability, his inclination, his interest and the press of other duties determine the time he spends on the work, pointing out to him that neither by direction nor by implication do I make any demand on him as to the amount of time he is to devote to this course. Here again he is compelled to exercise judgment and accept responsibility.

You will recall that each student makes his own selection of the number

of exercises which he is to study. To be sure this selection is to a slight extent limited by the grouping of the subjects from which he must choose, but where in this world are not our free choices of action more or less limited? At any rate, this freedom to choose what he will study, limited as it may be, has a very happy effect upon the attitude of the average student in approaching the work in the course. He feels that he is studying something that he has chosen to study. It is a mistaken notion, at least in biological work, that the use of one particular species of plant or animal or disease, or whatever, is essential for the demonstration of any principle, theory or general conclusion. Any professor who is so poor in his knowledge of illustrative materials as to feel that only one particular case will serve ought not to be teaching in a university. At least a sufficient wealth of materials should be available to the student's choice that he will have the feeling of interest in his work which comes from personal selection of subject-matter.

The requirement I make of the instructor in charge of the laboratory work that he shall answer questions and give assistance to students only when it is requested will doubtless cause head-shaking on the part of some of you. I would only reply that the universal practice of well-intentioned or meddlesome instructors butting into the student's cogitations with remarks on errors in drawings or other features of his laboratory activities does far more harm than any possible good which may accrue therefrom. When the student asks for assistance or information on a given point he is in the most receptive mood to receive what the instructor may have to offer on the matter. Many students need or require very little instructional assistance in their laboratory work. But, whether they need it or not, it should never be gratuitous. The student should seek enlightenment; it should not be thrust upon him.

The student is informed that he is always at liberty to come either to me or the assisting professor for help in his work at any time. I have no specified office hours for students. The student can always see me whenever and wherever he can find me regardless of what I am doing or with whom I may be in conference or consultation. I regard as my primary obligation that of assisting my students. No matter with whom I may be engaged, when a student appears in my presence with that look of wanting something, which we all know so well, I immediately excuse myself and attend to his wants, no matter if my visitor be the president of the university or the dean of the college. I have seldom found that students come to me with unwarranted demands upon my time or attention. In fact the only criticism I have of them is that they come too seldom and are too much impressed with my apparent professorial busyness. That teacher who is too busy to see his students has no business in a teaching staff. On rare occasions when the situation is such that I can not immediately attend to a student's wants I courteously ask his indulgence but always arrange for a time in the near future when we can conveniently consider the matter. As to the instructor and assistant, since they are graduate students and expected to give but a limited amount of time to teaching, students are informed that they are to be consulted only at the specified laboratory periods, and the instructor and assistant are directed to remind students of this if they are approached outside of these specified times and to send them to me or to the other professor. To be sure, instructors and assistants frequently disregard this command, but I wink at it with the pleasant feeling that they have gotten something

of the spirit of my own attitude toward students.

The conference feature of the course will doubtless arouse questions and doubts in your minds. Does it not require a great deal more time to handle a group of fifty students in this manner than in the old-fashioned way, some one will ask? Emphatically, no. Careful checking on this feature of the work in the early years under the new procedure as against that under the old showed that the conference system actually reduces the amount of time spent by the instructing staff on the course. The actual time devoted to teaching by myself and the assisting professor is twelve hours a week for each of us in the fall term and six in the spring, not counting the lectures and preparatory study. There is no great stack of note-books to be laboriously corrected at least once a week far into the small hours of the night either by professors, instructors or assistants, and what is more important, no need to make the customary pretense at doing so. The student brings to his conference his drawings and such notes as he may choose to make. The drawings are spread out on the table before the professor. He may quickly gather from this display before him the character and degree of accuracy of the mental pictures which the student has of the things he has been studying. These provide the teacher with a basis for many of the problems which he will set for solution, and they are presented at just that time when their presentation is most useful to both teacher and student. The student is not required to make drawings and may appear at the conference without them, in which case, however, he is usually asked to demonstrate on the blackboard that he does have accurate mental pictures of important features of the material which he has studied. There is no virtue in drawings as drawings. Their only value is in sharpening the student's mental picture of microscopic or macroscopic objects, features of importance in the materials which he had before him. Most students make the drawings for the professor, as may be easily proved by listening to their remarks and questions concerning them. Do we have to make these drawings? Just what do you want in this drawing? Is that what you want (exhibiting sketch)—and a dozen similar questions are daily thrown at the professor or instructor in most biological laboratories. Upon the first question of this sort, I always assure the student that "*I* do not want the drawing," that "if I wanted one of that particular subject, I could probably make a better one myself," that "at any rate, I could hire some one who can far outclass *you*." By such replies, kindly put, the student either tumbles to the fact that he is making the drawings for his own edification or he is driven to demand, why make drawings anyway? It is then at the moment of his insistent desire to know why, that I may profitably explain to him the value of careful and accurate drawings for him as means to sharpen and fix valuable mental pictures in his mind.

The time required for the preparation of fresh material, of which much is used in this course, is somewhat greater under the present procedure than it was under the old. Since not all the students who have selected a given disease will be working on it at the same time fresh material for each disease must be prepared in somewhat larger quantity and must be available over a considerably longer period of time. Furthermore, the practice of permitting each student to select his own list of diseases to be studied increases considerably the number of diseases for which fresh material must be prepared and kept on hand. But since the students are thus enabled to work at their own speed and on the

particular disease in which they are interested, the advantages far outweigh in value any saving of assistants' or instructors' time.

In fact, our present procedure requires very much less actual time of the assisting staff than does the ordinary laboratory procedure. There are no materials to be put out before the laboratory by a harassed assistant, rushing back from a hasty lunch. There is no gathering of scattered and wrecked specimens littering the laboratory after the students leave. Each student is responsible for the material he has checked out. He gets it out himself, he puts it away, and when at the end of his conference he returns the material to the materials room, it is carefully checked and any losses or injury to slides, specimens or other permanent material is charged to his breakage, and he pays for it. It is a grand and glorious feeling at the end of a strenuous laboratory period for the weary instructor to gaze upon the laboratory tables free of littered specimens and debris and ready without any effort on his part for the next period. The time thus saved to assistants and instructors in setting up and clearing away laboratory materials is more than ample to compensate for the additional work which they must do in preparing extra fresh materials.

A very important feature of the conference as we handle it is that of leaving to the student to decide when he wishes to take his grade. He is thus compelled to judge his own performance and to request the judgment of the professor as to the worth of his accomplishment. Being permitted a reconference as often as he chooses before accepting the grade leaves him little ground for a feeling of resentment over the grade which he finally receives. I am reminded of one student who, having made a rather poor exhibition, was asked if he was ready to take a grade.

"That was pretty rotten, wasn't it, Professor?" he said.

"Well, it might have been better," I replied, "but it is entirely up to you whether you take a grade or study the exercise some more, and come back for another conference, you know."

"Yes," he said, "but I am so far behind with my work now that I don't think I can afford to take the time to study this further. Guess I'll take my grade."

"All right," I replied, and went to the materials room to enter up the grade. He put away his materials and called out as he passed me, "Would you mind telling me what grade I got?"

"D," I said, "is the best I can do for you on that."

"Just about what I thought I'd get," he said in a disgruntled tone.

"My judgment was good, wasn't it?" I asked. A smile replaced the scowl on his face as he replied, "It certainly was."

In all our contacts with students whether in the laboratory or conference room we treat them as gentlemen and ladies. I never scold a student or bawl him out for poor work. If he is satisfied to do that kind of work and accept the consequent grade, that is his business. I have long ago divested myself of the feeling that poor work on the part of the student is any reflection on me, in spite of the fact that I may sometimes, of course, be responsible. I refuse to harbor any feeling that the student is doing the work for my benefit or glorification. On the other hand, I frequently ask the student after an exhibition of poor accomplishment if he would not be interested in knowing what I think is his difficulty. If he replies in the affirmative, I attempt in a kindly manner to explain to him where his difficulty lies, even to the extent of telling him that I think he is lazy. I have not yet brought myself to the point of telling a student

that I think he is dumb, even though I may be quite sure that is his malady. On the other hand, I never lose an opportunity to give a student a good word for an especially excellent performance. Praise is far more effective in stimulating a student than is harsh criticism.

Now a word as to what I think are some of the outstanding results from this method of procedure. Effective thinking, *i.e.*, facility in manipulating facts so as to arrive at logical conclusions, is, to my mind, the primary test of an educated person. Along with this, of course, comes the having of facts with which to think. A broadly educated man is one who has a good stock of facts on a great variety of subjects and is able to think with these facts intelligently and effectively.

The common practice of college professors in requiring primarily that a student shall accumulate facts to disgorge them on demand is, I think, one of the chief indictments of our college and university educational procedure. Bare facts are of themselves of no intrinsic value even though without facts the student can not hope to do any thinking. Facts are of value only as they afford materials for thought. It seems to me, therefore, that our constant aim should be not only to teach the student how most quickly and effectively to accumulate facts, but, what is more important, how to use these facts in thinking, that is, in solving problems and deriving conclusions, or in other words, making judgments. Too much emphasis in our college teaching is put on getting the facts but almost none on using them. To expect students to be zealous in the gathering of facts to be parroted back to the professor is absurdly naive. To take it for granted that students will effectively use the facts which they have accumulated without exercise in doing this exposes a childish faith in humanity quite inexcusable in a college professor. Yet nowhere in our usual teaching procedure is any provision made for giving the student exercise in the manipulation of the facts which he has learned.

It is in just this point that I feel our method of teaching makes its greatest appeal for approval, and this is borne out by the actual results as we watch the increasing facility with which the students in this course meet the problems which are presented for their consideration week after week. It is also proved by the growing eagerness and pleasure showing on the face of the student as he comes in succeeding weeks at the call for his conferences. At the first conference, hesitation and dread of the approaching ordeal usually show clearly on his countenance, but when he finds that the conference is only an opportunity for him to pit his wits against problems to be set for solution by a friendly professor, he comes to look upon the impending contest with the same joy of battle that inspires the football player or any other participant in a similar contest. I can not refrain from telling an incident in one of these conferences. Having set a rather difficult problem for a student with care in stating the conditions, I leaned back in my chair and awaited his operation. Plunging into the problem he was soon off on a false trail. I halted him with "Wait a minute, let me ask you a question." Judging he was not using a fact which he probably had in the back of his head I so set the question as to bring the fact to the fore of his mind. Immediately without giving the factual answer he replied, "Oh, yes, may I start again?"

"Certainly," I replied, and he was off now on the right track. Time after time, for a half hour or more I brought him up sharply with such a question, when I saw he was getting off the trail, and time after time he reacted in the same way with the reiteration, "Oh, yes, may I start again?" Finally, having worked

the problem out to a satisfactory and logical conclusion, he drew forth his handkerchief and wiped the streaming perspiration from his brow. "Pretty tough wasn't it," I said.

"Yes, but it was interesting," he replied with enthusiasm.

One marked advantage of this procedure is that each student can now work at his own speed and convenience. It is well known, of course, that no two students will cover the same ground in the same time. Some will wish to do only the essential minimum; others will want to explore various enticing side issues and matters of special importance to them. One does not usually study and learn to best advantage when he is constantly under the pressure of a time limit to his mental activities. The opportunity to suit his working hours to his convenience is also, I think, an important matter for the student. To be compelled to come to a laboratory on the afternoon following a wild night out or when he is half sick is not only irksome but futile in its results. It would always be much better for the student to sleep off his debauch or go to bed with his cold and come to his work later in a state of mental freshness. Under such conditions to come by compulsion at a specified time almost always results in a wasted afternoon. Nothing is accomplished, and the needed recuperation from loss of sleep or indisposition is denied him. He will generally leave the laboratory with the feeling that he has, at least, put in his time and so has met his responsibility in the matter, so that when mental alertness is again his happy possession he dissipates his energies in inconsequential activities with no feeling of responsibility for devoting them to acquiring the knowledge to which he has already paid the courtesy of an unprofitable gesture.

Another result of this procedure is that I now have all my time for real teaching. Much time and energy has been spent in working out the details of what I call the "machinery" of the course, in setting up the laboratory materials, *i.e.*, in preparing sets of specimens, photographs, etc., so that every student may have at his disposal the full quota of materials for each exercise. Careful thought has been given to the details of laboratory routine, so that no student will have to wait idly for any length of time in checking out specimens or in getting his conferences, etc. It is surprising how little hitches in matters of this kind will demoralize and nullify the teaching program, so that time devoted to their solution is time well spent in giving to the professor that complete freedom for effective teaching which the student has a right to expect. Assistants and instructors, trained and efficient in the routine of preparing satisfactory and effective materials, are an absolute requisite to this type of teaching. The excuse so commonly offered by professors that they do not have such assistance is not worth the breath it takes to make it. It is the professor's business to see that he has the necessary personnel and equipment for teaching. Otherwise, he should refuse to attempt the work and let the authorities above him take the responsibility. The responsibility for the common situation of an overburdened teaching staff, *i.e.*, a staff too small for the number of students taking the course, lies directly with the professor in charge. Firm refusal to accept more students for a course than can be properly taught with the staff available is not only the responsibility but the prerogative of the university or college professor. So long as he will accept more students than his facilities permit him to teach effectively he has no one to blame but himself. I would resign my job and work with my hands for a living before I would submit to such a situation. If all teachers in colleges and universities

would adopt this attitude and limit registration in their classes or offer their resignation as an ultimatum for the necessary facilities, the problem of poor teaching now so acute in colleges and universities would be largely solved. No really good teacher is likely to be fired from an institution for such action, at least, from an institution deserving of his services and devotion. As a result of adequate machinery I now come to my teaching with a feeling of enthusiasm and freedom from impending difficulties which I never knew before.

Another result is that I am always in a good humor when I come to my classes. There is nothing the student has done or can do to rouse my ire. There are no rules or regulations which I have made that the student can break and so irritate me. The student comes in the same frame of mind. He is not compelled to abide by any attendance regulations, nor must he accept my judgment of his work except at his own expressed desire for the same. Thus we both approach the hour together with the feelings of two friends who sit down together to pit their wits in pleasant and profitable contest. We meet as friends and part as friends. And finally, what is most important of all, the student has, at least, fifteen adequate opportunities during the term to exercise his mind with facts in the all-important educational process of solving problems, arriving at conclusions and making judgments on things in which he is presumably interested.

ELEMENTS OF ISOSTASY—OBSERVATIONS AND INTERPRETATION

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ISOSTASY is a comparatively new branch of geophysical science but a very important one. It is a condition of the outer portion of the earth which must be reckoned with when making investigations to disclose the cause or causes of the great changes in geographic positions and elevations of the earth's surface during geological times.

MEANING OF ISOSTASY

According to the idea of isostasy there is some depth below sea-level where the pressure exerted by the outer portion of the earth is the same on equal areas of a level surface at that depth. Above that depth there are variations in the density of the material, for in order to have equal pressures for equal areas the mass must be the same. Since the volume changes or varies the density must likewise vary. In other words, the product of the volume of a unit section of the earth's crust and the density of the material must be a constant around the whole earth above this depth, which is called the depth of compensation.

With a perfect isostatic earth and with no shifting of load on the earth's surface there would be no stresses present in the material of the earth below the depth of compensation. Necessarily there are stresses between that depth and the earth's surface, for there are great differences in the elevation of the solid surface of the earth in relation to sea-level. The Himalayan Mountains rise to elevations of more than five miles, and there are depths of water in ocean areas greater than six miles. There must be a stress difference exerted from a continent towards an adjacent ocean basin. This stress difference does not become zero until the depth of compensation is reached.

In computing the stress difference between a continental and an oceanic area necessarily the weight of the water must be taken into account. Correct values for the stress difference can be obtained by reducing the volume of water to that of an equivalent mass of surface rock. The density of sea water may be assumed to be 1.01, and a fair value for the density of surface rock is 2.70. A depth of water of about 2.7 miles is therefore equivalent to a layer of rock about one mile in thickness.

The depth of compensation has been derived from geodetic data and has been found to be of the order of magnitude of sixty miles. This depth has some uncertainty, but it is probable that the true value lies somewhere between forty and eighty miles below sea-level. It is my belief that the uncertainty in the derived depth is not greater than ten miles and that the depth lies between fifty and seventy miles. A knowledge of the exact depth is really not so important in geophysical and geological investigations as the idea that there is a depth at which the pressure of the superincumbent masses is the same for equal areas.

ISOSTASY IN INDIA

There are traces of the isostatic idea in some of the literature of one hundred or more years ago, but nothing very definite was set forth until the middle of the last century. John H. Pratt, when attempting to derive the figure of the

earth from triangulation and astronomical data in India, was struck by the lack of harmony in the observed data. He published the results of his investigations, and then George B. Airy, astronomer royal of Great Britain, gave an interpretation of the inconsistencies which had been found by Pratt.

On a hypothetical earth with a perfectly smooth mathematical surface the lengths of degrees of latitude would increase from the equator towards the poles. On the international spheroid the length of a degree along the meridian at the equator is 68.708 miles, and at the poles 69.407 miles. On an ideal earth if astronomical observations for latitude were made at places along a meridian and these places were connected by triangulation to furnish the distances between contiguous pairs of astronomical stations a gradual increase in the lengths of the degrees of latitude from the equator toward either pole would be found.

The Indian data showed great irregularities in the lengths of degrees of latitude and these were thought to be due, in large part, to the irregular surface of India and the areas to the northward. When corrections were applied to the astronomical observations to offset the attractive effect of the masses above sea-level the data were not improved. The corrections for the masses were greater than were needed to bring the triangulation and astronomical data into accord.

Airy suggested that the reason for this was that the crustal material varied in thickness and that under the continents it extended far down into the interior of the earth, which he supposed to be very plastic. There were according to him roots or protuberances of light material which floated the continental masses high above the dense material which lies under the ocean areas. He likened these continental masses to blocks of ice which float in water with a large portion of the block exposed. Pratt took exception to Airy's interpretation and advanced an alternative one. His idea was that the outer portion of the earth varies in density from place to place but that the variation extends to a uniform depth below sea-level. He questioned the validity of the hypothesis that there could be roots extending into subcrustal space below a mountain or plateau area.

Deflections of the Vertical and their Causes

For many years astronomers have been determining the right ascensions and declinations of stars and have collected data which enable them to predict the relative movements of the stars. The stars are not fixed. They wander around the heavens, but their movements are very slow. But even though the unaided eye would need centuries or perhaps thousands of years to detect any real difference in angular distance between any two stars, yet with the high-grade instruments used in the determination of latitude and longitude a star is found to have a different position each time it is observed. The work of the astronomer enables the geodetic engineer to compute the exact declination and right ascension of a star for any instant of time, and from these data he is able to determine accurately the latitude and longitude of a point on the earth's surface.

As is well known, the surfaces of the oceans of the earth have the general form of a spheroid of revolution. If there were no irregularities in the earth's surface and the densities were normal everywhere, this water surface would be a true spheroid of revolution. Since the earth's surface is irregular the continents tend to deflect the plumb line, to which all astronomical observations are referred, from the direction at

right angles, or normal, to the mathematical surface—the spheroid.

It is possible to determine the astronomical latitude of a place with a probable error of 0.10 second of arc or less, while in longitude determinations the probable error is seldom greater than .03 second of time. The actual error in linear measure in the determination of an astronomical latitude is believed to be seldom greater than about twenty feet.

We now know the shape and size of the earth with a fair degree of accuracy. With this knowledge we are able to start from an astronomical station and measure distances and directions across a continent by means of triangulation and to compute the positions of the stations on the adopted spheroid. Triangulation, as is well known, is a system of surveying based on the elementary mathematical principle that if the length of a side of a triangle and the two adjacent angles are known the lengths of the other sides can be computed. In practice a side of a triangle called a base, varying from five to fifteen miles in length, is measured directly by means of steel or invar tapes or wires. The probable error in the length of a base derived from direct measurements is seldom greater than one part in a million, which is all the accuracy required. At intervals of one hundred miles or so, depending on the character of the terrain, additional bases are measured in order to check the lengths carried through the triangles by computation.

From the triangulation data one is able to compute on the spheroid the latitude and longitude of each of the triangulation stations. Any errors in the geographic positions are due in part to the uncertainty in the adopted values for the dimensions of the earth and in part to errors of observations in the triangulation. These errors are comparatively small. If the astronomical latitude and longitude are determined for a number of the triangulation stations, a comparison of the astronomical and geodetic positions at each of these stations gives the values of the deflections of the vertical.

The deflections of the vertical may amount at some places to thirty seconds of latitude or longitude or even more. The linear distance on the earth's surface corresponding to half a minute of latitude is about one half mile. This is so large that accurate surveying and mapping can not be based on a number of detached astronomical stations. The triangulation connecting these stations gives the distances with great accuracy, and therefore the geographic positions determined by geodetic methods are used in the surveying and mapping of continents and in the charting of the waters along the coasts. The map for any particular continent or country is really based on an average value for the latitudes and longitudes which are determined astronomically. Latitudes, as is well known, are merely the angular distances to the north or south of the equator, and longitudes are the angular distances to the east or west of some fundamental meridian. The initial meridian in use generally over the world to-day is the one through the observatory at Greenwich, England.

The deflections of the vertical are due mostly to the irregular surface of the earth and the isostatic compensation of the topographic features. When corrections are applied for the effect of topography and compensation there remain residuals or unexplained portions of the deflections. These residuals are undoubtedly caused by variations from normal density in rock comparatively near the astronomical stations. There may be a large trough filled with unconsolidated sediments near a station, in which case the residual deflection would be away from this area. On the other hand, there may be an outcrop of Pre-

cambrian rock underlaid near the earth's surface by igneous rock. This material is denser than normal and therefore the residual deflection of the vertical will be towards it. Buried structure is indicated by the residual deflections, and therefore geodetic data in the form of astronomical determinations of latitude and longitude and the latitudes and longitudes determined by geodetic means may be of great assistance in both theoretical and economic geology.

Isostasy in the United States

The equilibrium hypotheses of Airy and Pratt received only scant attention for some years. In 1889 Clarence E. Dutton, a member of the U. S. Geological Survey, advanced practically the same ideas as Pratt and Airy regarding the equilibrium of the earth's crust. He seems to have derived his ideas from a study of geological phenomena only. He refers only casually to the work done by geodesists in connection with the triangulation and astronomical data of India. Dutton's exposition of the idea of isostasy started discussion and arguments, but it is rather remarkable that he had very few active followers. Most of the students of the earth of his day were opposed to his views.

During the past few decades rather extensive tests have been made of isostasy, and we now can say with assurance that isostasy in its general aspects is true. Much more must be done in the way of detailed investigations and observations to learn if possible the exact depth of compensation; whether the Airy or the Pratt idea is the true one, and how large in horizontal extent is the block of the earth's crust which can be taken as the unit of mass. It is certain that geodesists and geophysicists will work intensively on these problems, and it is expected that in the next decade much more will be learned of the details regarding isostasy.

Maintenance of Isostatic Equilibrium

In order properly to explain isostasy and the geological processes one should begin at the time of the origin of the earth when it became a separate astronomical body, but this would require more space than is available in this paper. We may assume that the earth has approximately the same size now that it had when it first had a solid surface; or to put it another way, its shape and size now are approximately what they were at the beginning of geological history, which presumably was at the beginning of rainfall, erosion and sedimentation. We are vitally interested only in the processes which have shaped the earth's surface since the beginning of sedimentation and which now are continually changing it. We need not know what was the original condition of the earth, whether it was a molten mass or solid throughout in order to be able to study the processes which are now in operation.

The work of the geodesist and the seismologist proves very conclusively that the outer portion of the earth to some limited depth (the geodesist has found it to be approximately sixty miles) is solid and that it resists the stress differences which tend to flatten out the surface into a smooth mathematical one. The results of their observations and analyses of the data secured indicate that the earth is solid from the crust down to a great depth below sea-level. There is still some uncertainty as to the physical condition of the materials near the center. This involves about an eighth of the earth's volume. The transverse or elastic earthquake waves will not go through that core. If the earth were elastic throughout the elastic waves would pass through the center. The conclusion seems to be that the core of the earth is either a liquid or a non-elastic solid.

The proof of isostasy leads to the definite conclusion that the material below the crust, that is, below a depth of about sixty miles, is plastic to long continued stresses. The observations of earth and ocean tides, of the variation of latitude and of seismic phenomena indicate that the earth as a whole is solid and has a rigidity about twice that of steel, or if the core of the earth is a liquid then the remaining solid part of the earth has an even greater rigidity. These observations, however, involve forces which change phase rapidly. They do not act in the same direction for great lengths of time. It is possible, of course, for a material to be rigid to short continued stresses but to yield if stresses are applied for a long time.

The reason why we conclude that the material below the crust acts as if plastic to long continued stresses is that all land areas in which isostatic investigations have been made show a high degree of equilibrium. Certain areas may have been subjected to erosion for a long time but the pendulum observations and deflections-of-the-vertical data indicate clearly that they are not lighter than normal. Similar data indicate beyond doubt that portions of the crust covered by thick beds of recent sediments do not have more mass than normal.

We may assume that the normal mass is contained in a block of the earth's crust whose surface elevation averages about zero. Such blocks are along the margins of continents. The blocks of the crust which are undergoing erosion must be lessened in mass as a result of the removal of portions of their surface material, but there is an influx of subcrustal matter into crustal space to offset the loss. This lessening in weight or mass would surely be detected by an analysis of the geodetic data in the region in question if there had been no upward movement and addition of subcrustal matter. There is only one way for a block of the earth's crust undergoing rapid erosion to maintain its normal mass or weight, and that is by an intrusion of subcrustal material into crustal space. The block of the crust becomes light as the result of erosion and is buoyed up and elevated by the plastic material which lies beneath it.

In areas of sedimentation along the margins of tidal waters, or even in lakes or valleys, the crust yields beneath the added load and there is a sinking of crustal material into subcrustal space. Since the weight of the eroded material is equal to that of the material deposited as sediments there can be a restoration of the isostatic balance by a horizontal movement of subcrustal material from beneath the area of sedimentation towards the one undergoing erosion.

The Earth a Yielding Mass

All the above may seem very strange, for we are inclined to think of the earth as a very unyielding solid mass. But gravity and deflection-of-the-vertical data are accurate, and the conclusions drawn from them seem to be able to withstand the most critical analysis. The seismologist tells us that the earth is not a rigid body, that is, it is not rigid to long continued stresses. Not an hour passes during which there is not an earthquake somewhere on the earth. It has been estimated that there are approximately eight thousand earthquakes recorded annually on the seismographs now in operation. The number of seismological stations is quite small and many earthquakes of a local and minor nature are not recorded. It would be mere guesswork to say how many earthquakes occur during a year, but it is reasonably certain that the number is several times eight thousand. Earthquakes are caused by breaking or slipping rock. A few may be due to explosions near volcanoes, but these are rather small when measured by the en-

ergy involved. The large earthquakes are the result of movements of crustal material without any accompanying explosion.

If the earth can have thousands of earthquakes per year it is surely not a fixed body, that is, it is not fixed in form and in the relative positions of its particles. Movements must be going on continuously, although in most places the rate of movement is so small that it would take years with refined methods and measurements to detect it.

We do not have to depend on earthquake and geodetic data to show that the earth has been unstable in past geological times and is now unstable. There are few places on the earth's land surface which do not show that they were at a former time below sea-level. This is indicated, of course, by the presence in the rocks of fossils of marine life. Even on the slides and slopes of the highest mountains marine fossils can be found. Areas which are now occupied by the Himalayas, the Alps, the Rockies and the Andes were in an earlier geological time below tidal waters. These areas now have mountain peaks standing high above the sea. Even though it has been some millions of years since they were below sea-level, yet geologically speaking the rate of elevation of those mountain areas has been quite rapid. Areas once occupied by the mountains which furnished the sediments that were later raised into the present mountain masses now lie near or even below sea-level and are receiving new masses of sediments.

The best geological evidence available indicates very definitely that all mountain and plateau areas were once areas in which great masses of sediments had been deposited. There must therefore be some process operating in the earth which is closely connected with sedimentation and mountain and plateau building. It seems evident that the mass of some blocks of the earth's crust is made smaller by erosion and that of other blocks is made larger by sedimentation provided there is no movement of subcrustal material to restore the balance.

The earth's crust must be very weak to yield to the forces involved in erosion and sedimentation, for it is certain that a deficiency or excess of load or mass amounting to a disk of surface rock of indefinite horizontal extent and a thousand feet thick could be detected easily by means of gravity data. There are geological evidences that ten thousand or more feet of material have been eroded from some areas and that thirty thousand feet or more of sediments have been deposited in others. Using a thousand feet of material as the minimum amount that could be detected by geodetic evidence we should still have to conclude that isostasy is in a high state of perfection. I am convinced that the masses of the earth's block do not deviate even a thousand feet from normal. A disk of a thousand feet of material of indefinite horizontal extent would cause a variation from normal gravity of about thirty parts in a million. No such deviation from normal gravity occurs over any large land area which has been investigated.

Study of Gravity Anomalies by Grouping Stations

There are, of course, gravity anomalies which are larger than thirty parts in a million, but the area within which such large anomalies obtain is small. A brief report entitled "Isostatic Condition of the United States as Indicated by Groups of Gravity Stations," Serial No. 366 of the Coast and Geodetic Survey, shows very clearly that the average anomaly for a portion of the area of the United States about 350 miles square is quite small. In fact, there are only a few sections of the country of that size where the average anomaly is more than

about fifteen parts in a million or the equivalent in mass of a disk of rock 450 feet thick. This is an exceedingly small amount of material as compared with the great masses which have been eroded from mountains and plateaus and carried to tide waters and to lakes and valleys.

The mean of the average gravity anomalies for the thirty-eight squares or sections into which the country is divided is only −.004 dyne. This of course shows that the United States as a whole is not underlaid by an excess or deficiency of crustal material. Since one part in a million is the attraction of a disk only thirty feet thick, four parts in a million is equivalent to only about 120 feet. This, however, does not represent a true departure from isostasy, for the theoretical formula on which the anomalies are based may not be absolutely true and this residual may represent an error in it rather than a measure of the departure from equilibrium of the crust under the United States.

Explanation of Gravity Anomaly

A gravity anomaly is the difference between the observed value of gravity and the theoretical value. If the earth had a perfectly smooth ellipsoidal surface and if for any latitude the densities along radii varied in the same way, then the variation of gravity on the surface would follow a very definite law. The actual sea-level surface of the earth approaches very closely to an ellipsoidal surface, and gravity observations are referred to it. The change in the value of gravity with change of elevation is well known, and a correction can be applied to the observed gravity to reduce it to sea-level under the point of observation. Corrections can also be computed and applied to the observed value of gravity for the effect of the topographic masses, which are the masses above sea-level or the deficiency of mass in the ocean waters. Finally a correction can be computed and applied for the effect of the isostatic compensation or the deficiency in density in the crust below mountains and islands and the excess of density in the crust beneath ocean areas.

If these corrections are applied to the theoretical value of gravity at sea-level at the latitude of the station the difference between the resulting value and the observed value is the gravity anomaly. Sometimes the corrections referred to are applied to the observed value of gravity in order to reduce it to sea-level below the point of observation. In this case the difference between this corrected value and the theoretical value at sea-level at the latitude of the station is the gravity anomaly. The anomaly has the same value in either case of course.

Studies of Isostasy in Canada

Some of the most valuable geodetic data used in isostatic investigations have been obtained by the Dominion Observatory, Ottawa, Canada. That organization has made a gravity survey over parts of Canada, and a recent paper by Mr. A. H. Miller, of the observatory, entitled "Gravity in Western Canada," gives data for sixty-nine gravity stations. Many of these stations are in high mountainous areas, and it is very remarkable that the anomalies are exceedingly small even though the average elevation of the stations is great. As a matter of fact, there are forty-three stations of the sixty-nine which have elevations greater than 1,000 feet, while sixteen stations have elevations more than 2,500 feet. There are only three stations at elevations above 2,500 feet which have anomalies more than twenty parts in a million.

The data secured by Miller lead one to believe that the mountain areas of Canada are in a high state of equilibrium. Gravity data in mountainous

areas in other parts of the world lead to the general conclusion that mountain masses are not extra loads added to the crust beneath but that they are balanced by a deficiency of mass in the crust under them. Where there is so much topography, that is, mass above sea-level, a small anomaly is an indication of only a small percentage of departure from the true isostatic state. But even these small anomalies are not positive evidence of a real departure from isostasy, for they may be due to irregularities in density distribution near the gravity stations. They may also indicate an erroneous value for the computed topographic effects as the region near a station may not have been completely mapped.

Miller made a computation of the depth of compensation from his gravity data at twenty high stations in the Rocky Mountains. He concludes the discussion of the depth of compensation with the following sentence: "Evidently the most probable depth of compensation for the twenty stations is between 85.3 and 113.7 kilometers, possibly slightly less than 100 kilometers." The most probable value for the depth of compensation derived by an analysis of the deflection and gravity data in mountainous regions of the United States is ninety-six kilometers, or approximately sixty miles. It is rather interesting that this value and Miller's should agree so closely.

Determination of Depth of Compensation

It is impossible to obtain a correct value for the depth of compensation by the use of gravity stations which are on extended plains or in wide interior valleys. If there were no errors in the observations for gravity and if there were no other causes for the anomaly than the compensation of the topographic features then it would be possible to derive a depth which would have a fair degree of accuracy from plains stations and stations where the average elevation around the station is not great. We know, however, that the cause of at least a large part of the gravity anomalies is the effect of masses of material close to the stations which have abnormally heavy or light densities.

Where the average elevation of the topography close to the station is great then there is a large effect of this compensation on the value of gravity. Even though there might be abnormally heavy or light material close to the station, the effect of the compensation would overbalance this, and we should be able to obtain a depth value which would have much strength. If we should take many gravity stations in high areas then the effects of the local divergences from normal densities on the various stations would tend to balance out, and the derived value for the depth of compensation would be very strong and probably very close to the truth.

At Seattle, Washington, which is quite far from large masses of topography, the gravity anomaly, after the isostatic reduction has been made with a depth of compensation of 113.7 kilometers, is ninety-three parts in a million of gravity. The elevation of the Seattle station is less than two hundred feet above sea-level and the attraction of the topography in the vicinity of the station is not more than about six parts in a million. We can see that no change in the depth of compensation used could materially reduce the anomaly, which is many times six parts in a million.

It was mentioned above that the station must be surrounded by an area having large masses of topography. This statement applies to stations which may be at sea, either on ocean islands, over the margins of continental shelves or

over great ocean deeps and submerged ridges. What are really needed are large masses of topography and irregularities in the elevations or depths of this topography in order that a fairly accurate value may be obtained for the depth of compensation.

The method of determining the depth of compensation has been criticized by some opponents of isostasy. We assume that the crustal material extends to a uniform depth and then compute the effect of the isostatic compensation distributed uniformly to various depths differing successively by several miles. From the data secured we are able to determine the anomalies for each of the several depths used. That depth should be the most probable one which makes the sum of the squares of the anomalies a minimum. The method of determining the depth of compensation is set forth in some detail in Special Publication No. 40 of the Coast and Geodetic Survey.

This method of determining the depth of compensation may enable us to tell whether the Airy or the Pratt idea of isostasy is the true one. Eventually we may secure a great amount of gravity data in each of the great mountain systems of the world. If the Airy idea is the correct one we should obtain derived depths which will vary directly as the average height of the mountain area. There is more topography to be compensated for in the Himalayas than in the Rockies, and therefore if mountain masses are upheld by roots those extensions into subcrustal space should be greater for the Himalayas than for the Rockies. We shall have to wait for the solution of the question until we have vastly greater amounts of gravity and deflection data in the areas covered by the great mountain systems.

Horizontal Extent of Compensation

One of the problems of the future will be to learn whether an area of small extent is underlaid by crust which is not of itself in isostatic equilibrium. It is certainly true that a mountain peak such as Mt. Shasta or Pike's Peak is not compensated for by a deficiency of material immediately below. If the compensation is strictly local then each column of material a few hundred feet or a few miles in diameter, extending from the surface to the depth of compensation, must have a deficiency of mass equal to the mass above sea-level. Undoubtedly there is regional compensation of topographic features such as mountain peaks and mountain ridges, but I do not believe that the compensation that balances such features extends for great distances around them. The data available at present do not enable us to derive the exact distances to which the compensation extends, but after considering all geological and geophysical facts the most probable conclusion is that the compensation does not extend as far as fifty miles around any topographic feature. However, this is not based on definite evidence. It is hoped that future data will enable us to throw light on this very interesting subject.

Denudation and Sedimentation

The student of the earth necessarily is anxious to solve the problems involved in the great changes which have occurred on the earth's surface during geological time. In the absence of definite information we can only guess at many of the possible causes. Some of these guesses may be close to the truth and some may be far from it. In any event, we know one cause which is surely a fundamental and important one. That is the shifting of masses over the earth's surface as a result of denudation and sedimentation. These processes work slowly when reckoned in terms of the length of human life but in terms of geological time they are most important.

The amount of rain to-day over the land surface of the earth averages about

thirty inches per year. At this rate a mile of rain would fall in about two thousand years. In a billion and a half years the amount would be approximately three fourths of a million miles of rain. We use a billion and a half years because that represents, according to best geological and geophysical evidence, the time elapsed since geological processes as we know them to-day first began their operations. The beginning of the sedimentary age of the earth was a long time ago. Sediments of course can not be carried without running water and an irregular surface of the earth, and running water requires rain. We may therefore conclude that rain began falling about a billion and a half years ago, that it probably has continued to the present time without interruption and that in the beginning the earth's surface was irregular.

Rainfall causes an enormous amount of denudation. According to measurements of the U. S. Geological Survey the amount of material carried from the United States to tidal waters is the equivalent of one foot from the whole surface of this country in nine thousand years. At this rate a thousand feet of erosion would occur in nine million years, or a mile in forty or fifty million years. During the sedimentary age, at this rate, there might have been thirty miles of erosion, but of course no such amounts could have been eroded from a given region, because even the highest areas like the Himalayas would be reduced to sea-level long before any such amounts were eroded.

According to the isostatic idea much more material must be eroded to base-level a mountain area than exists above sea-level at any one time, because as erosion continues there is a rising up of the subcrustal material below to restore the isostatic balance. If the difference in density of the surface and the lower portion of the crust is 20 per cent. there should be an upward movement of the crust of eight hundred feet as one thousand feet is eroded from the surface. An area would be decreased only two hundred feet in average elevation by the erosion from it of one thousand feet of material.

As sediments are laid down the crust beneath surely sinks under them. Some of the sinking is due to causes that are independent of the weight of the sediments, but undoubtedly the pressure of the sediments is an important factor in the movement.

The isostatic condition of the crust seems to be maintained almost to perfection. This condition has probably obtained during the entire sedimentary age. If this is true then the crust of the earth must give way to stress differences caused by denudation and sedimentation. Undoubtedly the maintenance of the isostatic balance is a very important factor in geology. Much has been written in geological literature of the crushing, folding and overturning of strata in uplifted areas. In general the authors have either stated or led the readers to infer that most of this uplift and distortion occurred during the growth of the mountain systems. In my judgment much of this movement and distortion has been due to the moving upward of the earth's crust under areas of erosion to restore the equilibrium.

Since under sediments the earth's crust is depressed to maintain the equilibrium, it is reasonable to suppose that much of the breaking and folding of strata occurs as the sediments are laid down. Sedimentary material is usually deposited irregularly along a coast. At one time a river's mouth may be at a certain place on the coast and at another time some thousands of years later it may be a hundred miles or more away. This irregularity in time and place in the way the sediments are laid down leads to irregularity in the strata.

Although the records of the U. S. Geological Survey indicate that the rate of denudation from the United States is equivalent to one foot in nine thousand years, this does not represent all shifting of material in our area. Much of the eroded material from a high mountain area or from a dissected plateau region does not reach tidal water. It is deposited in the foothills or out on the plains just beyond. Apparently a much more rapid rate of erosion takes place from a region of high elevation than from the area of the United States as a whole. This leads us to believe that the rate of uplift in a mountain area to restore the isostatic balance is far greater than that for the United States as a whole.

Mountain Formation

Many mountain areas have been elevated and then later depressed below sea-level. In some cases this has occurred several times. Do we not here find a correlation between the erosion and the sedimentation on the one hand and subsidence and elevation on the other? An area which in one geological era is receiving sediments and being depressed and in a later one is being elevated shows that the placing of sediments must have some effect on its changes in elevation.

For the first mile and a half below the earth's surface we know that the temperature is increasing with depth. During sedimentation the earth's crust is depressed an amount approximately equal to the thickness of the sediments. Every part of the depressed crust goes into a zone whose normal temperature is higher than that of the zone previously occupied. Later on there is undoubtedly some uplift of the sedimentary area due merely to the normal thermal expansion as the crustal material takes on its new temperature. But this thermal expansion would be only approximately three thousand feet even for a depression of thirty thousand feet, and this estimate is based on the assumption which can not be strictly true that there would be no heating of the crust while it was sinking under the weight of the sediments. There must therefore be some other reason besides the normal thermal expansion to cause an area to be elevated a mile or more following a period of sedimentation.

Some areas which have been high and subjected to denudation are now low or even below sea-level. Here we have the opposite process to that which affects the crust under an area of sedimentation. As denudation progresses the crust beneath is pushed up by subcrustal material to restore the balance. This brings the crustal material into higher and presumably colder zones. As the moving material assumes the temperature normal to its new position the decrease in temperature causes it to contract. A depression of the surface results and a trough may be formed into which new sediments tend to be deposited. The depression of such an area is probably much more than would be caused by normal thermal contraction.

The depression and the uplift of areas must be due to a large extent either to some chemical or physical changes which cause increased or decreased densities of crustal material, or to forces acting tangentially in the earth's crust which tend to compress certain areas and to elevate them, and to expand and depress other areas. The writer believes that the changes in elevation, aside from those due to normal thermal expansion and contraction, are due to chemical or physical processes which change the density of the crustal material.

Recently the writer had a conversation with Professor Charles P. Berkey in which the question of isostasy and of the maintenance of the isostatic equilibrium of the earth's crust was discussed in some detail. We agreed that erosion

and sedimentation are factors in changing elevations of the earth's crust, but Professor Berkey held that denudation and sedimentation are not the most important factors in the uplift of mountains and plateaus. He holds that the story of such uplifts could be told if we knew more about volcanism and of the behavior of igneous rocks. I can subscribe heartily to that view. Undoubtedly volcanism is a very important factor, but the question of whether volcanicity, or that which causes it, is the principal factor involved in uplift is debatable. As is shown above, the pushing down of the crust into hotter zones by the weight of sediments and the movement upward of the crust to restore the balance under areas of denudation may be a primary cause with volcanicity as the secondary one. In any event, to cause an uplift of the earth's surface to the extent of one mile involves a change in density of the material of the crust throughout the sixty miles of thickness of less than 2 per cent. Surely this is not a very large change of density, and it might well be caused by some change of physical or chemical state due to changed temperature and pressure brought about by denudation and sedimentation.

The earth's crust must be very weak to permit the isostatic condition to be maintained so perfectly. Thousands of earthquakes occurring every year indicate that rock is being broken under stress and strain. It is difficult to see how the earth's crust can at one place be under tremendous horizontal compressive stresses resulting in an uplift and not far away be subjected to horizontal tensional stresses resulting in a depression. As a matter of fact, uplift and sinking are going on at the same time in the earth's crust, for where a mountain area is being formed in a region that was previously subjected to heavy sedimentation a trough is being formed where the mountains once stood that furnished the sediments now being uplifted. Surely the earth's crust as a single unit can not at the same time be subjected to both compressive and tensional horizontal strains world-wide in extent.

Relation of Gravity Anomalies to Geological Structure

When the gravity data are corrected for the effects of topography and compensation and the anomalies are obtained this information can be applied to certain geological problems. It has been found that the isostatic gravity anomalies in general are due to abnormal densities close to the gravity stations both horizontally and vertically. Where stations are placed close together around a disturbed area the space occupied by abnormally light or heavy material can be outlined in a general way.

There are several instances in the United States where the gravity anomalies and the geological structure seem to be in complete accord. A notable example is the eastern margin of Puget Sound. Thirty or more years ago a station was established at Seattle and the anomaly, or unexplained difference between the observed and theoretical value of gravity, was found to be ninety-three parts in a million, which corresponds to the attraction of a disk of surface rock of indefinite horizontal extent and about 2,800 feet in thickness. The block of the earth's crust under Seattle seemed to have a deficiency of mass equal to that amount. The opponents of isostasy some years ago made much of this large Seattle anomaly, claiming that it indicated that the northwestern part of the United States is abnormally light. When additional gravity stations were established in the Puget Sound region, however, the data secured showed very clearly that there is a narrow strip running north and south along the eastern

margin of Puget Sound in which the gravity anomalies are all negative and rather large in amount. On the western margin of the sound the gravity anomalies are practically zero or normal. In a comparatively short distance east of Puget Sound the gravity anomalies become very much reduced in size. All this indicates that there is a deep trough filled with recent material running along the Puget Sound region. This deduction is substantiated by the opinions of geologists that there is a vast amount of recent sedimentary material along the eastern margin of the sound. The data do not indicate, therefore, any decided departure of the earth's crust from the isostatic condition in the Seattle region.

There is a large gravity anomaly at Minneapolis, Minnesota, at a station which for several years stood alone, with no others within about two hundred miles of it. The large anomaly, fifty-nine parts in a million, seemed to indicate that the region around Minneapolis is too heavy and therefore out of balance with the rest of the earth's crust. Later several gravity stations were located around Minneapolis, and these did not show large positive anomalies. In fact one of them at Baldwin, forty miles to the eastward, has a large negative anomaly. Several years afterwards a well was sunk in the vicinity of Minneapolis and at a comparatively shallow depth a ledge of very heavy rock was discovered. At Baldwin the geological evidence seems to indicate the presence of a large body of recent sedimentary material. There are many cases where gravity stations placed close together indicate buried structure. Therefore it would seem possible to depend on gravity observations in reconnaissance surveys made to disclose at least the major features of buried structure. In this way gravity observations have a value in both economic and theoretical geology.

There is much more that could be said in regard to the importance of keeping isostasy constantly in mind when one tries to solve the great structural and dynamic problems of geology. Much has already been written on the subject, and the literature of the future will undoubtedly treat isostasy at even greater lengths than has been the case in the past.

HUMAN BIOLOGY

By EDWIN R. EMBREE

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As I read the twenty-seven manuscripts which together went to make up the volume "Human Biology and Racial Welfare," I found myself thinking of Clarence Day's essay which he entitled "This Simian World." He considered what kind of a planet this might be if some other species than the great apes had evolved into mastery. What dignity and wisdom might have been displayed if children of elephants instead of monkey-like animals had developed into leadership; what cleanness and cunning in a world ruled by super-cats; what poise and wisdom in the glorified descendants of eagles! But as a matter of fact, animals akin to monkeys were the ones who did evolve; it is the children of that race who rule the earth today. The biology derived from this ancestry governs our potential development and marks its ultimate borders.

We inherit some very great liabilities from these animal forebears. Our bodies are weak and puny as compared with the magnificence of elephants. The grace and beauty of the great cats is lacking in our simian civilization. We have little sense of personal dignity and no real regard for privacy. We congregate in hordes, live together crowded into tenements and hotels. We are unstable, constantly running after new toys and new ideas, rushing, often aimlessly, up and down the earth as our ancestors used to scuttle chattering among the trees.

But we inherited in common with our monkey cousins one great talent, namely, curiosity. And that single quality, probably more than all other things taken together, is responsible for the phenomenal progress of our race. We have an insatiable hunger to know all about everything. This appetite drives us to avid gossip about our fellows; to handling and tinkering with—"monkeying with"—every object or idea that crosses our path; to rushing hither and yon to glimpse a dog fight or view an aeroplane, and also to deep and profound study of intricate problems of medicine and physics.

Two other characteristics have helped us humans in our special type of progress. Our chattering forefathers have given us a love of talk. We are forever gabbling; we have invented great systems of language; we even pay men to talk to us in groups. We build huge temples called libraries in which to hoard this preserved chatter. We compel children to devote years to the study of talk of previous generations. We have invented devices whereby we can speak to our friends thousands of miles away, and machines which record our babble and reproduce it. This ability to talk and our devotion to it is a biological character of our species. It enables us to communicate ideas as well as gossip and to pass on to the whole race our accumulated research and experience.

We have also inherited a compulsion to action. We must always be busy; we rush about; we build and tear down and build again. We are not content simply to inquire and find out everything, but we are driven to do something about it all. This, again, while it means a lot of aimless motion, also results, for example, in turning our knowledge of physics into bridges and steam trains and aeroplanes, and our knowledge of chemistry and medicine into protection of health.

The twenty-seven collaborators have themselves provided evidence in support of Day's thesis. Each of them is a distinguished investigator, driven by a

"'satiable curtiosity" for facts and explanations. Then having made their explorations they must tell of them; and in the telling they disclose their interest in the application of their facts and theories. Fortunately the desire to tell is strong, and it has led them to write far more easily and entertainingly than is common in scientific treatises. Fortunately, too, their desire to write popularly has not outweighed a painstaking regard for the facts.

What these twenty-seven men of science have joined to tell us is the kind of world we live in, the kind of creatures we are, the probable limits of our future development and the extent to which the human race, by its own effort, can hope to change the world of to-day into a more desirable world to-morrow.

It is important that in such fundamental matters we proceed, wisely, cautiously and on the basis of well-established facts. Any constructive activity in human biology must rest upon the carefully assembled findings of wise research and must be supported by intelligent public opinion. Monkeying with the universe is risky business on any other terms. Indeed, in many minds, the danger outweighs all other considerations. Though human progress has been a series of triumphs over natural forces, certain people can be counted upon to cry out against any new proposal, however well founded upon fact and however carefully considered. They call these proposals "perversions of nature." Of course they are. Man rules and always has ruled by bending the world to his will.

Man rules, in so far as he does, because he has turned nature to his service. Natural science is a series of victories over other animals and over inanimate forces. Coal, in the normal "state of nature," lies in deep pockets underground; electricity naturally is jumping haphazard about the universe. Man has lured the bees to store up great piles of sweet food, not for themselves, but for man. Cows, that by nature furnish milk for their young, he has perverted into continuing their supply of milk long past the needs of their calves. He has exploited the seed-bearing nature of fruits and grains. He has crossed one species with another and produced such hybrid foods as the tangelo grapefruit. He has developed to a state of perversion the normal tendencies of many vegetables so that larger, richer roots grow on Burbank potatoes, more profuse grain on many varieties of wheat and oats, larger and more succulent stalks on sugar-cane. He has interfered with the natural reproduction of animals in order to breed cattle with greater quantities of muscle for him to eat, and hens with a penchant for laying eggs. He has produced abnormalities such as oxen and mules where these better serve some special purpose of his.

Man also interferes with nature when he kills parasites which might otherwise cause his illness or death. He changes natural processes when he gives anesthetics to deaden pain and when he aids childbirth. The whole story of medicine is a history of triumphs over natural forces. And now man is beginning to take an interest in even more vital elements of control. He practices birth control; he makes it impossible for certain of the insane or feeble-minded to reproduce their kind. He is beginning to inquire about the possibilities of breeding not only better horses and dogs, but even a finer race of men. Against such proposals many cry, "It is a perversion of nature." Certainly; but no more so than flying in aeroplanes, using milch cows, growing grapefruit or wiping out the cause of yellow fever.

"Human Biology" is a book for those who dare to look at the world as "biological statesmen." No book could contain all the basic facts and theories, but I know of no other volume which presents so many of them or presents them with greater authority and charm.

RAMÓN Y CAJAL—AN APPRECIATION

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At the corner of the Paseo de Atocha and the Calle de Alfonso XII in Madrid is the Museo Antropológico, with its pillared portico and domed roof. In front of the *museo* are two statues in stone, one of them of Michael Servetus, who lived in the sixteenth century, and whose history is well known to all physiologists and anatomists. Adjoining the *museo* is a three-story building, not unlike neighboring houses, with arched entryway and balconied windows, and on the third floor is the laboratory of Professor Santiago Ramón y Cajal, the fame of whose researches has extended to all parts of the scientific world. Such are the present surroundings of this great investigator, who has done more than any one else to make clear the cellular structure of the nervous system. The illustration on page 179 is from a photograph taken in the library of the institute in the spring of 1928, while I was studying there. In this room and in the small one adjoining it much of his recent work has been done. The library itself is worthy of special comment. The walls are lined with books on all sides from floor to ceiling. On one of the shelves back of the table where the master sits is a row of books embodying the researches of himself and his pupils. To these he often refers the student for some point in question or topic to be elaborated. The laboratory consists of a series of more or less connected rooms, and here the research workers have their tables and the technicians make the silver preparations. The equipment, both in the way of microscopes and of photomicrographic apparatus, is modern, mostly German or French, and the chemicals used are of the best. There is a feeling of optimism and cheerfulness in the air, as if past achievements were the best augury for future successes.

The wide-spread influence of this Spanish school of neurohistology may be readily gauged by looking into books of histology, anatomy or physiology, and seeing the number of illustrations credited to Cajal.

Cajal's most comprehensive work is his "Textura del sistema nerviosa del hombre y de los vertebrados," published in 1899–1904. This was translated into French by Dr. Azoulay and published by Maloine et Cie. in Paris in two volumes in 1909–11. In this book he gives the results of his personal researches on the histology of all parts of the nervous system. One of the collateral factors in the success of the comprehensive survey which he was able to make was his choice of animal for his work. He made great use of the mouse, one of the smallest of mammals, and by using the young of this form as well as of other forms he was able to follow through all the structures of the brain in a comparatively small number of microscopic sections. Also, because of the contiguity of the various nuclei, he could often see nerve processes extending to their destination. Thus he was able to see the architecture of the mammalian brain in a way nobody had been able to see it before. Following him, many have investigated special parts of the brain, and time and again have found themselves merely elaborating what Professor Cajal had already observed.

Early in life the young Cajal showed a decided taste for drawing. Indeed, he would have liked to become an artist and devote himself to art alone. And so, when he began to study osteology and

PROFESSOR RAMÓN Y CAJAL IN THE LIBRARY OF THE INSTITUTO CAJAL

later to examine his microscopic preparations, he was able to make fine drawings, and to this capacity for illustrating his own papers must be ascribed some of his success in making the world realize his discoveries. Always there is boldness of delineation, with delicacy of detail, which bespeaks clarity of vision and well-considered judgment.

Professor Cajal has provided us with a delightfully intimate account of his career in an autobiography entitled "Recuerdos de mi vida," the third edition of which was published by Juan Pueyo, Luna 29, Madrid, in 1923. Here he gives us in vivid fashion the events of his early life, his education, his military training in Cuba, his aspirations and enthusiasms, and also a good account of his achievements. Certain parts of the opening chapters of this have been arranged as a reading text in Spanish for the use of American college students, and published by Henry Holt in 1925. The best introduction to Cajal's work is to study this survey which he himself has given us. In more than one hundred full-page plates, he shows us in chronological order the results of his numerous studies.

One of the earliest subjects was the structure of the cerebellar cortex, and to this he has repeatedly returned. His first study of it yielded important discoveries, and he then elucidated for the first time the internal structure of the cerebellum. He was able to see (1888, 1890) the endings of the nerve fibers bringing impulses into the cerebellar cortex. These he designated by the descriptive terms of climbing and mossy fibers, and as a result of this discovery he was able to portray the pathway of nerve impulses through the cerebellar cortex, and the schema which he constructed you will see copied to the present day in practically all books treating of this topic.

In 1889 Cajal attended the October meeting of the Deutsche Anatomische

MUSEO ANTROPOLÓGICO, MADRID
IN THE BUILDING ADJOINING IT ON THE LEFT IS THE INSTITUTO CAJAL.

Gesellschaft in Berlin. On his journey, he visited Frankfurt-am-Main, among other scientific centers, and there met Weigert and Edinger, of the Neurologisches Institut, as well as Paul Ehrlich. At Berlin he demonstrated his preparations of cerebellum, retina and spinal cord to the assembled anatomists. Among these were His, Schwalbe, Retzius, Waldeyer and Koelliker. There were beautiful preparations of axones of granule cells of the cerebellum, pericellular baskets, climbing and mossy fibers, the bifurcations and ascending and descending branches of the sensory spinal roots, the long and short collaterals of the tracts of the white substance, the terminations of the retinal fibers in the optic lobes, etc. After this convincing demonstration he received many sincere felicitations. The most interested of all was perhaps Koelliker, the venerable patriarch of German histology. After the demonstration he took charge of Cajal, entertained him at dinner at his hotel, introduced him to many of the most notable German histologists and embryologists and exerted himself to make his stay in the Prussian capital agreeable. Koelliker was especially interested in the minute details of the methods employed, and after the meetings went back to Wurzburg and tried out the methods as used by Cajal. In this way he was able to make successful preparations and to substantiate and uphold Cajal's work. After this a knowledge of Cajal's name and achievements spread rapidly through the scientific world, and it was recognized that a new star had arisen.

An idea of his rapid progress and of the readiness with which the scientific world acclaimed his results may be gained from the record of the many invitations and honors received from foreign countries. In 1894 he was invited to give the Croonian lecture before the Royal Society in London; in 1899 he came to America to take part in the celebration arranged by Clark University to mark the completion of its first ten years; in 1906 he shared with Camillo Golgi one of the Nobel prizes. On each of these occasions he made addresses in which he summarized the results of his recent research, and these give a clear

HOME OF THE INSTITUTO CAJAL

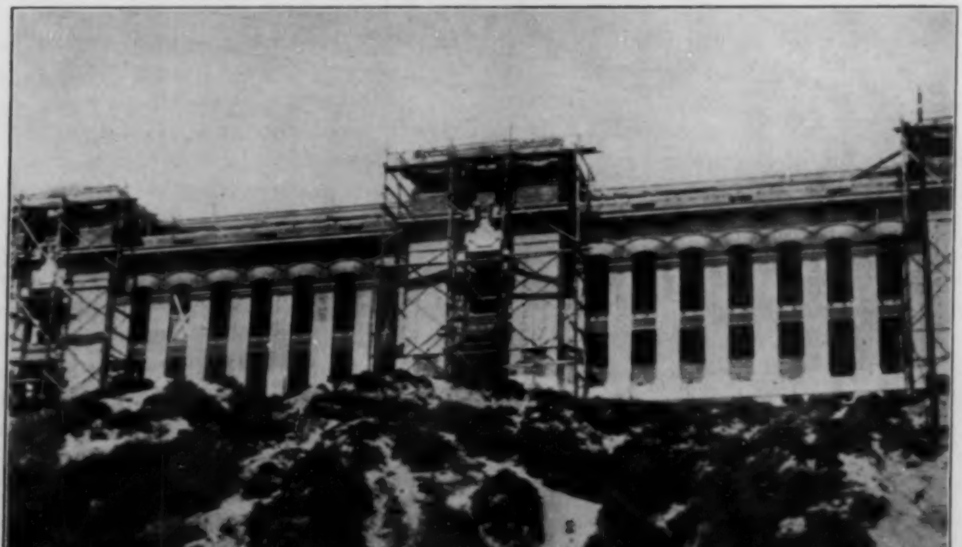
indication of the continued trend of his work.

In 1894, when he was in his forty-second year, he delivered the Croonian lecture of the Royal Society of London. This is a signal honor accorded only to investigators of the first rank. Virchow had been the lecturer of the preceding year, and Koelliker and Retzius on earlier occasions. It was not without some hesitation that he accepted the invitation. However, the cordiality of the secretary of the Royal Society, Michael Foster, and of his host, Charles Sherrington, made the visit pleasant and agreeable. His title was "La fine structure des centres nerveux," and the lecture was printed in full in the *Proceedings* of the Royal Society, volume 55, 1894. A good abstract in English appeared in the *British Medical Journal* at the same time, and also a brief account of Cajal's career. The lecture was given on March 8 at Burlington House, with Sir John Lubbock in the chair. In order to follow the discourse each one in the audience was given a printed abstract of the more important points. By the aid also of photomicrographs and of large colored charts, Cajal was able to demonstrate his observations in a clear and convincing manner. After referring to the newer histological methods, he described the minute anatomy of the cerebral nerve cells, and traced some of the pathways for nerve impulses within the brain, especially of the olfactory and visual systems. He showed how the impulse within a nerve cell passed from dendrites to cell body, and from cell body to axone, introducing the idea of the dynamic polarity of the neurone, which is regarded to-day as a well-established law.

Cambridge University conferred on him the honorary degree of doctor of

science on March 5. The official orator made the presentation address in Latin. In it he referred to the use of gold and silver in unraveling the delicate filaments of the human body, and concluded with a selection, slightly altered, from the Spanish-born Roman poet Martial. All this was highly entertaining to the guest of honor. At the dinner of the Royal Society he was acclaimed with great enthusiasm, and Michael Foster declared in his eloquent discourse that "thanks to the work of Cajal, the impenetrable forest of the nervous system had been converted into a regular and pleasing park." He was showered with all forms of hospitality. Also he was taken about the hospitals and medical schools, and regarded himself as fortunate in seeing physiological experiments in the laboratories of Ferrier, of Horsley and of Mott, and examining the histological preparations of Schaefer and of Sherrington.

In 1899 Cajal was invited to take part in the decennial celebration of the founding of Clark University. He was one of five distinguished Europeans selected to give addresses embodying the results of their own researches. The others were A. Mosso, professor of physiology at Turin; E. Picard, professor of mathematics at the Collège de France; A. Forel, of the University of Zurich, and L. Boltzmann, professor of theoretical physics at the University of Vienna. He arrived in New York at the beginning of July and found the heat suffocating, much hotter than Madrid, and the heat pursued him into New England. At the celebration he gave three discourses on "The Comparative Study of the Sensory Areas of the Human Cortex," a topic on which he had been working during the years 1898 and 1899. In the audience, he tells us, there were chiefly physicians, naturalists and psychologists. The lectures were illustrated by large wall charts done in colors, and to those especially interested in neurological technique he gave a demonstration of his microscopic preparations. Cajal's lectures were delivered in French, but were translated for publication, and they together with the other lectures and the proceedings of the



THE NEW INSTITUTO CAJAL

meetings were printed in a memorial volume—"Clark University, Decennial Celebration," 1899, issued by the university. These lectures of Cajal were illustrated by thirty-one figures, and are important contributions to our knowledge of the subject. His closing paragraph strikes a prophetic note of more than passing interest:

> I can not conclude this, my third and last lecture, without a word of tribute to this great people of North America—the home of freedom and tolerance—this daring race whose positive and practical intelligence, entirely freed from the heavy burdens of tradition and the prejudices of the schools which still weigh so heavily on the minds of Europe, seems to be wonderfully endowed to triumph in the arena of scientific research, as it has many times triumphed in the great struggles of industrial and commercial competition.

After the meeting he visited the educational institutions of Boston and New York, and also took a trip to Niagara Falls. He saw much to interest him, and has written in the "Recuerdos" an entertaining account of his impressions of life in America.

In 1900 the International Congress of Medicine, meeting in Paris, awarded him the Moscow prize. This was given for those medical achievements, published within the preceding three years, which had rendered the greatest service to science and humanity. This was direct testimony to the high place he already occupied, not only in laboratory but also in clinical science.

Perhaps the greatest international honor attainable is to receive the Nobel prize. In 1906 the prize in physiology and medicine was divided between Santiago Ramón y Cajal, of Madrid, and Camillo Golgi, of Pavia. On December 12, 1906, Cajal delivered his address at Stockholm on "Structures et connexions des neurones." On the preceding day Golgi had given his address, "La doctrine du neurone, théorie et faits." Already, on December 10, the king had presented the prizes to the several recipients in the presence of the royal family, the diplomatic corps and a distinguished gathering of literary and scientific men. His introducer said that Cajal by his numerous discoveries had given to the science of the nervous system the form which was accepted at the present time, and that by his researches he had laid the foundation on which would be built the future developments of this branch of science. At the banquet Professor Sundberg proposed an enthusiastic toast to Professor Cajal. Cajal responded, also in French, and recalled among other things the memory of his illustrious predecessors, the pioneers in the science of histology.

Each time Cajal was called away from his laboratory he received high honor, and now he had achieved the pinnacle of scientific recognition. But his life work was only well begun. He was already having success with his new methods of reduced silver for demonstrating the neurofibrils within the neurones, and from these he advanced into new methods for showing the structure of the neuroglia more definitely. In the application of these methods he was aided by his students, who exhibited the same enthusiasm as the master. As a result, the Spanish school of neurohistology has contributed as much to the knowledge of the neuroglia as of the neurones.

Madrid has already honored herself in erecting a statue of Cajal. As one walks in the *retiro*, it is with surprise and pleasure that one recognizes the features of the master. In bas-relief on either side are symbolic figures of birth and death, which with the adjoining fountains make an impressive setting.

On a height not far from the present institute is a building still under construction which it is hoped will soon be the home of the Instituto Cajal. In the meantime, in that beautiful and historic city of Madrid the present *instituto* is a shrine which all neurologists and workers in medicine are sure to seek.

AERIAL PHOTOGRAPH OF THE MOUNTAINS OF OREGON AND WASHINGTON

THE PROGRESS OF SCIENCE

AERIAL PHOTOGRAPHY BY INFRA-RED RAYS

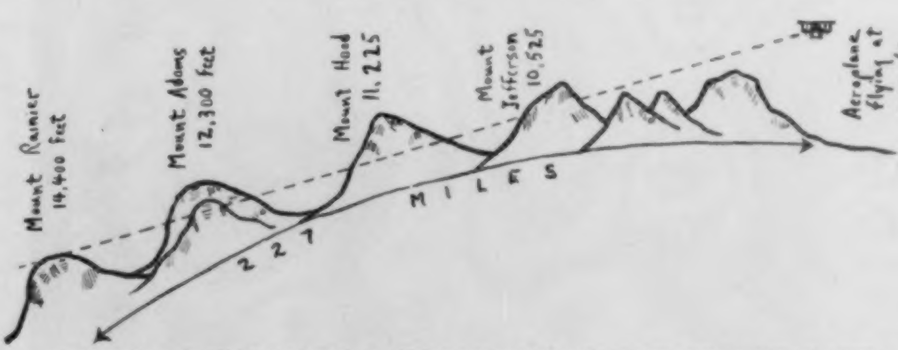
The accompanying photographs were taken last August by Captain A. W. Stevens, of the U. S. Air Corps, from an airplane flying at an elevation of seventeen thousand feet. Mount Rainier in the state of Washington was 227 miles north, the pictures having been made from a point well south of the center of the state of Oregon. The distant mountains are beyond the range of the eye and are recorded on the photographic plate by the infra-red rays. Even on clear days the atmosphere contains sufficient moisture and dust particles to limit vision, and the infra-red rays penetrate the smoke and haze to a greater extent than the shorter rays of the visible spectrum.

Experts at the Eastman Kodak Research Laboratories have made a thorough study of haze penetration and have found that by using special emulsions and color filters results can be obtained which greatly eliminate the ever present haze factor. Aerial photographs of good quality have been taken on specially sensitized material showing objects which are nearly invisible to the naked eye because of the obscuring haze.

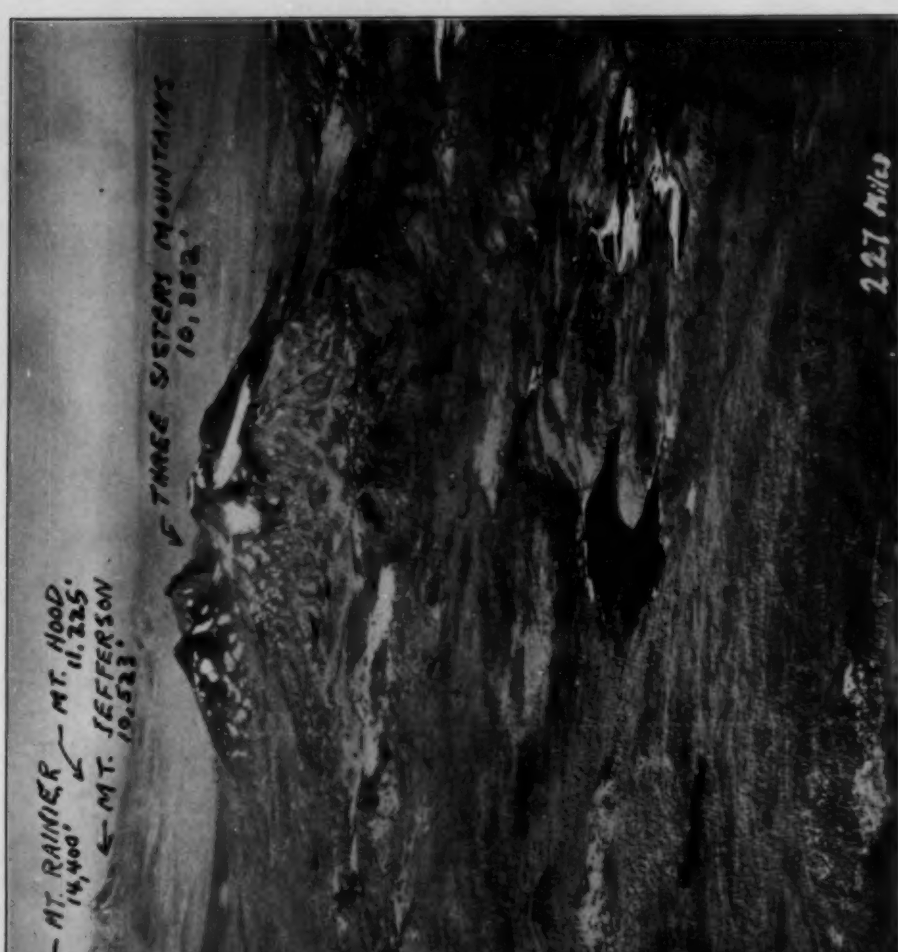
Haze is a phenomenon with which nearly every one is familiar. It is almost always present in the atmosphere to some degree. In taking pictures from high altitudes there necessarily exists a depth of air between the camera and the objective which reflects strong actinic light back to the lens. This light can usually be described as a haze or thin veil of a bluish or violet cast. As the predominating color of haze lies in that part of the spectrum which is most sensitive to ordinary photographic emulsions an emulsion which is sensitive to all visible colors and even to a part of the invisible spectrum, the infra-red, must be used.

A dye called kryptocyanine has been found which will sensitize a photographic emulsion in the infra-red end of the spectrum. Panchromatic emulsion which is sensitive to all colors is bathed with this dye to make it sensitive beyond the visible spectrum.

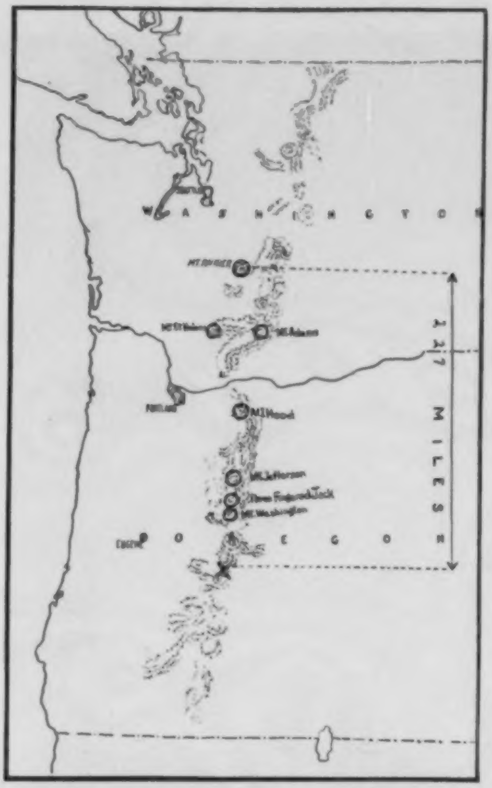
By using a strong yellow filter the violet and bluish light reflected from the haze can be eliminated. This, however, is not all the correction needed. Green foliage on panchromatic film with kryptocyanine added will photograph almost white. A red filter must also be used to tone down the green reflection and render foliage more natural. Thus, by using a film so sensitized and a strong yellow and red filter before the



CONTOUR OF THE EARTH AS PHOTOGRAPHED FROM THE AIRPLANE



AERIAL PHOTOGRAPH SHOWING MT. RAINIER AT A DISTANCE OF 227 MILES



MOUNTAINS OF OREGON AND WASHINGTON SHOWING POSITION OF THE AIRPLANE 227 MILES FROM MT. RAINIER

lens, it is as if the emulsion sensitivity started at the farther end of the blue part of the spectrum and gradually increased as it neared the infra-red.

The aerial pictures taken by Captain Stevens were on panchromatic film which had been hyper-sensitized by bathing in a solution of ammonia and made sensitive to the infra-red by the addition of kryptocyanine. Between the components of the 36-inch focal length lens a strong yellow filter has been cemented. This made possible the penetration of the violet bluish atmospheric haze. A red filter corrected the effect produced by the kryptocyanine which renders green too light. This filter will hold back the light reflected from foliage and provide a more natural coloring. Captain Stevens used a time of exposure as short as one thirty-fifth of a second.

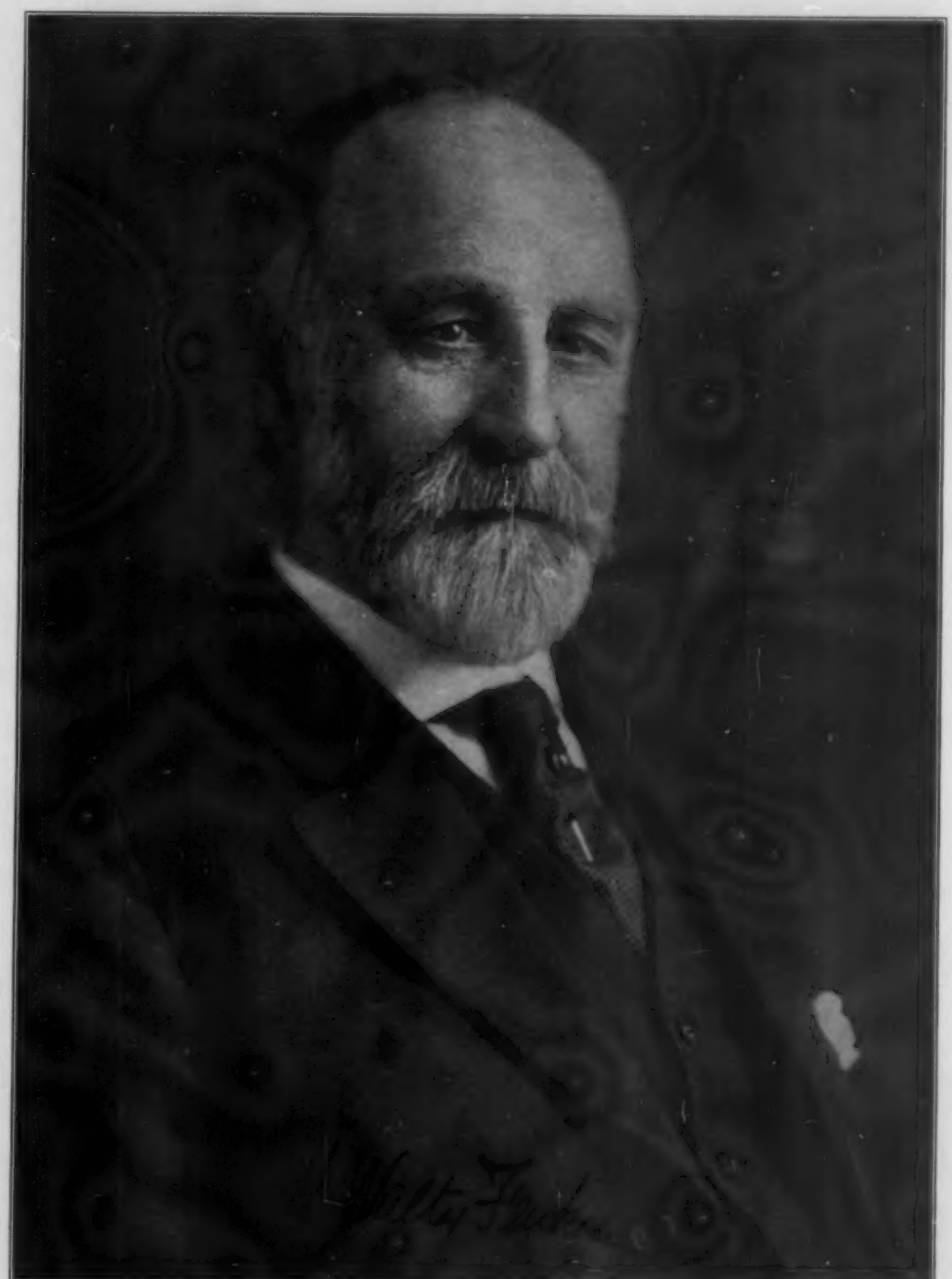
There is little likelihood of ever being able to photograph through dense layers of haze since radiation reflected from the haze body is often greater than that reflected from the earth and transmitted by the haze. Therefore when any one speaks of photographing through several miles of mist or fog he is either misapplying the terms "mist" and "fog" to what is usually considered haze, or he is working on the credulity of the general public which has not yet become familiar with the process.

Another interesting adaptation of the use of infra-red photography has been found by astronomers. Dr. W. R. Wright at the Lick Observatory has photographed Mars on ordinary emulsion and with the infra-red emulsion. On the former the planet's atmosphere is clearly visible and on the latter the Martian surface is boldly defined. By superimposing one photograph on the other it is possible to measure the extent of the atmosphere of this planet.

CAPTAIN A. W. STEVENS, U. S. AIR CORPS

DR. HARVEY WASHINGTON WILEY

FROM 1883 TO 1912 CHIEF CHEMIST OF THE U. S. DEPARTMENT OF AGRICULTURE, LEADER IN THE MOVEMENT FOR PURE FOODS AND DRUGS, WHO DIED ON JUNE 30. THE PHOTOGRAPH WAS TAKEN TWO YEARS AGO WHEN DR. WILEY WAS EIGHTY-THREE



J. WALTER FEWKES

LATELY CHIEF OF THE BUREAU OF AMERICAN ETHNOLOGY, DISTINGUISHED FOR HIS CONTRIBUTIONS TO INDIAN ARCHEOLOGY, WHO DIED ON MAY 31, IN HIS EIGHTIETH YEAR

PORTRAIT BUST BY GEORGE LOBER OF DR. EDWARD GOODRICH ACHESON

THE WORK OF DR. EDWARD GOODRICH ACHESON

The United States National Museum has installed an Edward Goodrich Acheson exhibit in its Arts and Industries Building. Rising from the center of a rectangular table is a pedestal on one side of which are grouped a portrait of Dr. Acheson and a number of the medals awarded to him at various times during his career. Below these is a series of three printed labels containing a biographical sketch of Dr. Acheson, and arranged on the floor of the table are mementoes such as his notebook of experiments and scientific observations; business cards and letters when he was associated with Edison; a copy of his paper on "Lightning Arresters" presented before the American Institute of Electrical Engineers in 1889, and his book of tank capacity calculations, compiled and recorded when he was twenty-two years old.

The other three sides of the pedestal and floor of the exhibition case contain material bearing on Dr. Acheson's inventions and discoveries of carborundum, artificial graphite, colloidal graphite and Egyptianized clay. Descriptive labels explain the particular properties of these materials; files containing samples familiarize the visitor with their appearance, and suitable objects indicate their applications to use. The carborundum section of the exhibit includes the original analysis of this substance made by the Pittsburgh Testing Laboratory in 1892; a reproduction of the plumber's pot furnace with which Acheson first produced carborundum; photographs of early and modern electric furnaces; some early carborundum advertising literature, and a few modern carborundum products.

Dr. Acheson was born on March 9, 1856, at Washington, Pennsylvania, where his grandfather, coming from Ireland, settled in 1788. In 1880 he entered the employ of Thomas A. Edison at Menlo Park, New Jersey, as an assistant draftsman under John Kruesi. In July, 1881, he was sent as first assistant engineer of the Edison interests at the Electrical Exposition in Paris. For a time he was connected with the Consolidated Lamp Company of Brooklyn and the Standard Underground Cable Company of Pittsburgh.

Early in 1891, Dr. Acheson produced in the electric furnace the first sample of carborundum, the well-known abrasive and refractory now used all over the world. The invention of carborundum has been of great benefit to many industries, cheapening the production of numerous articles and improving the finish of many others.

Not long after the invention of carborundum Dr. Acheson produced the first large sample of artificial graphite. He took ordinary coke and heated it to extremely high temperatures in his electric furnace which transformed the hard, brittle coke into a soft, unctuous product, graphite. And this graphite he thus produced artificially was better and purer than the natural mineral "plumbago" or "black lead" as natural graphite is commonly called. To-day about twenty million pounds of Acheson graphite are produced annually. Graphite has many uses, among them being lubricants, electrodes and crucibles. The tungsten wire of the incandescent lamp owes its existence to "aquadag," an aqueous lubricant of "deflocculated Acheson graphite." The range of our guns was increased twenty per cent. by surfacing the inner walls with Acheson graphite. The Acheson product is so fine that it will pass through a chemist's filter paper which ordinarily retains the finest precipitate.

Dr. Acheson's career has been fascinating and fertile. Untiring in his zeal and highly skilled in the experimental art, he has transformed into realities, as though by magic, the visions of his active and imaginative mind—realities that are products of inestimable value to science, to the arts and to civilization.

EXPLORATIONS OF THE DEEP SEA

Dr. William Beebe, director of the Department of Tropical Research of the New York Zoological Society, and Mr. Otis Barton have just completed a number of dives in a deep sea chamber or bathysphere in the open ocean to a depth far beyond where any scientific observations at first hand have ever been made. This bathysphere was designed for and financed by Mr. Barton, who in frequent consultation with Dr. Beebe has worked on it for a year. The barge from which the dives were made was kept anchored off Nonsuch Island where the New York Zoological Society Oceanographic Expedition has its headquarters, and the descents were made in the open sea in connection with and within the limits of the area of intensive research which Director Beebe has carried on for two years past.

The length of cable at the greater depths was checked and rechecked both by hydrographic meterwheel and by measuring off and marking one hundred foot lengths of the cable, the difference between these methods being two feet in one thousand four hundred and twenty-six.

On June 6th a descent was made to 803 feet, and on June 11th to 1,426 feet, or beyond a quarter of a mile, with both Beebe and Barton in the bathysphere. The sphere is 57.3 inches in outside diameter and $1\frac{1}{2}$ inches thick, and at the greatest depth withstood a pressure of 652 pounds to the square inch, or a total of 3366.2 tons on the whole surface. Fifteen dives have been made altogether.

The two most surprising phenomena were, first, the abundance of life observed, and the clarity and certainty with which it could be seen and identified, and second, the blue brilliance of the watery light to the naked eye, long after every particle of color had been drained from the spectrum. Another unexpected fact was the presence of fish and invertebrates at these upper levels which, in trawling nets, have been taken only hundreds of fathoms lower. At 700 feet the spectrum, as seen close against the quartz window, was quite devoid of color, the lightest portion being at the 510th wave-length. The last color to disappear was violet, which, many feet above, had completely overlaid the blue.

A strong, electric search-light illuminated the outside water to a distance of many feet, a bag of decayed fish and baited hooks served as a lure, an outside thermometer was easily read, and perfect telephone communication and dictation were carried on without interruption.

Luminous fish and shrimp swam close to the quartz window, about a dozen species of true bathypelagic fish being identified and seen again and again. Puzzling results of the trawling nets were explained, and every possible ecological fact noted and dictated.

A second important phase of the work proved to be dangerous but exceedingly interesting. This was to lower the bathysphere in shallow water, and as the guiding vessel slowly drifted seaward to do contour exploration down the Bermudian insular shelf. The risk was the possibility of suddenly sighting a wall of reef too near to be cleared by reeling the sphere quickly upward. Four such descents, to a maximum of 350 feet, yielded unexpected results, revealing an entirely new fish fauna at these offshore depths and opening a new field for the study of the unknown bottom life connecting the shore with the deep sea faunas. The most notable thing about the recognizable shore fish was their great average size.

The satisfactory margin of safety and the ease and accuracy of observations of fish and other bathypelagic organisms have ensured the advisability of the continued use of the bathysphere for another year.